THE
PHILOSOPHICAL MAGAZINE,
OR
ANNALS
OF
CHEMISTRY, MATHEMATICS, ASTRONOMY, NATURAL HISTORY, AND GENERAL SCIENCE.

BY
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AND
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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

VOL. I.

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ERRATUM.

Page 260, *for* 200,000, *read* 206265; the number by which the length of a division on the scale of a level answering to 1″ must be multiplied to obtain the length of the radius of its curvature.

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JANUARY 1827.

I. *On the Elastic Force of Steam at different Temperatures. By* J. Ivory, *Esq. M.A. F.R.S.**

THE principal questions relating to steam are these two: 1st, What is the relation between the elastic force and the temperature? 2dly, How much heat is required to produce steam of a given elasticity or temperature? I propose at present to treat of the first of these questions only; and my intention is not confined merely to seek a numerical formula, but, if possible, to investigate some property or law, that may instruct us, generally at least, in regard to what takes place beyond the present range of our experiments.

The best experiments on the expansive force of steam are those of Dalton, comprehending all temperatures between the freezing and the boiling points; and those of Dr. Ure, which extend from the freezing point to 312°. We have also, more lately, a Table of results by Mr. Philip Taylor, from 212° to 320°†, which added to Mr. Dalton's Table completes the same range of temperature as the experiments of Dr. Ure. The three Tables we have mentioned are nearly on equal footing in point of accuracy; but in a research like the present, it seems better to adopt, as the foundation of our reasoning, the results obtained by one observer, and by the same uniform processes. For this reason the experiments of Dr. Ure are made the ground work of the following Table:

* Communicated by the Author.

† Phil. Mag. Dec. 1822.

Index.	τ	e	Log. $\frac{e}{30}$	t	$\frac{\text{Log. } \frac{e}{30}}{t}$	Diff.	Computed quantities.	
								e
0	50°	0·360	−1·29082	−162	·011857	...	·011857	0·360
1	70	0·726	−1·61618	−142	·011381	476	·011403	0·721
2	90	1·360	−1·34359	−122	·011013	368	·010968	1·378
3	110	2·456	−1·08689	−102	·010656	357	·010553	2·634
4	130	4·336	−0·83704	−82	·010208	448	·010158	4·408
5	150	7·530	−0·60032	−62	·009682	526	·009783	7·424
6	170	12·05	−0·39613	−42	·009432	250	·009428	12·05
7	190	19·00	−0·19837	−22	·009017	425	·009092	18·93
8	210	28·88	−0·01652	−2	·008260	...	·008777	28·81
9	230	43·10	+0·15736	+18	·008742	...	·008482	42·63
10	250	61·90	+0·31457	+38	·008278	464	·008206	61·50
11	270	86·30	+0·45889	+58	·007912	366	·007949	86·70
12	290	120·15	+0·60260	+78	·007726	186	·007714	119·9
13	310	161·30	+0·73051	+98	·007454	272	·007497	162·8

In this Table the column marked τ contains the temperature beginning at 50°, and increasing continually by 20° as far as Dr. Ure's Table enables us to go. In the column on the left are placed the indices which denote the number of intervals of 20°. If τ denote any temperature indefinitely, and x the corresponding index, we have,

$$x = \frac{\tau - 50}{20}.$$

The next column, marked e, contains the elasticities, or the tensions of the steam, in inches of mercury, taken from Dr. Ure's Table. Immediately after are placed the logarithms of the elasticities estimated in parts of an atmosphere of 30 inches. Then follows the temperatures of the steam reckoned from the boiling point, which are negative for all cases below 212°, and positive for all cases above it. In the next column are placed the quotients of the numbers in the two last columns. These quotients are irregular near 212°; because as $\frac{e}{30}$ approaches to unit, its logarithm varying rapidly for any change in e, the errors of observation have a great influence in this part of the Table. It is remarkable however that the numbers in this column form a series continually decreasing. Supposing the Table to be continued, would the numbers go on decreasing to a fixed limit? Or, would they decrease to a *minimum*, and then increase again? The differences of the quotients are placed in the next column. These differences are extremely irregular, and, taken directly, seem to furnish

no

no clue to guide us in our present research. But we may gather in general that they decrease slowly; and hence we may infer that the quotients, at least for a considerable range of temperature, may be expressed with tolerable accuracy by means of the first and second orders of differences. But as these differences cannot be found directly, we must try to deduce them in the best way we can from the numbers in the Table. Representing the first and second differences by Δ and Δ^2, we have this general expression of the quotient corresponding to the index x, viz.

$$\frac{\log.\frac{e}{30}}{t} = \cdot 011857 - x.\Delta + \frac{x^2 - x}{2}.\Delta^2. \qquad \text{(A)}$$

Two values in the Table answering to given indices, are sufficient for finding Δ and Δ^2; but, on account of the irregularities of observation, it will be better to proceed as follows. Form the expressions of the seven quotients in the Table, corresponding to the indices 1, 2, 3—7, and take a mean of the whole; then,

$$\cdot 010198 = \cdot 011857 - 4\,\Delta + 8\,\Delta^2.$$

In like manner, form the expressions of the four last quotients, and take a mean; then,

$$\cdot 007842 = \cdot 011857 - \frac{23}{2}\,\Delta + 61\,\Delta^2.$$

By means of these two equations we get,

$$\Delta = \cdot 0004545$$
$$\Delta^2 = \cdot 00001986.$$

Having now found Δ and Δ^2, we must return upon our steps and compute, by means of the formula (A), the several values corresponding to the indices 1, 2, &c. The results of these calculations are placed in the next column of the Table, and it appears from inspection that they are surprisingly near the real quotients. Using the computed quotients in place of the real ones, the elasticities have been calculated and set down in the last column of the Table. Thus, to find the elasticity answering to the index 4, we have the equation,

$$\frac{\log.\frac{e}{30}}{-82} = \cdot 010158;$$

whence, $\log\frac{e}{30} = -\,0\cdot 8329 = \bar{1} + \cdot 1671;$

then,

$$\begin{array}{lr} & -1\cdot 1671 \\ \log.\ 30, & \underline{1\cdot 4771} \\ \log.\ e, & 0\cdot 6442, \end{array} \quad e = 4\cdot 408.$$

In like manner have the other elasticities been calculated,

and the differences from the experimental quantities are insignificant. It appears therefore that, taking the values of Δ and Δ^2 which have been found, the formula (A) represents the elasticities as exactly as can be wished. In order to reduce it to a proper form for use, I substitute the values of Δ and Δ^2, and arrange the terms according to the powers of x; then,

$$\log. \frac{e}{30} = \cdot 011857 - \cdot 00046443 . x + \cdot 00000993 . x^2.$$

Now we have, $x = \frac{\tau - 50}{20} = \frac{162 + t}{20}$;

wherefore, by substituting, Logarithms of coefficients.

$$\begin{aligned} \log. \frac{e}{30} = & \cdot 0087466\, t \;.\;.\;.\;.\;.\; -3\cdot 9418393, \\ & - \cdot 000015178\, t^2 \;.\;.\;.\; -5\cdot 1812202, \qquad \text{(B)} \\ & + \cdot 000000024825\, t^3 \;.\;.\; -8\cdot 3871228. \end{aligned}$$

At the freezing point, $t = -180$, and the elasticity by the formula comes out 0·185, which is not sensibly different from 0·2 the experimental quantity. The formula (B) may therefore be considered as very nearly exact in the whole range of Dr. Ure's experiments.

Beyond Dr. Ure's Table there are only two experiments that I am acquainted with which deserve notice. The first is by Mr. Southern, who makes the elasticity equal to 8 atmospheres, or 240 inches of mercury, at 343°·6. In this instance $t = 131\cdot 6$, and the elasticity computed by the formula is 264 inches, or 24 inches above the experiment. If this appear a very great difference, it is to be observed that it corresponds to a variation of 6°·6 of the thermometer; for, by the formula, the elasticity is exactly 240 at 337°, when $t = 125$. It is to be observed too that Mr. Southern and Dr. Ure differ from one another in the temperatures of the elasticities which both have determined by their experiments, as will thus appear:

Elasticity. inches.	Mr. Southern. temp.	Dr. Ure. temp.
60	250°·3	248
120	293 ·4	290
		Formula.
240	343 ·6	337

We may therefore conjecture that the formula does not err much at the great pressure of 8 atmospheres.

The other experiment is by M. Clement, who makes the elasticity equal to 35 atmospheres at 419°. Now here $t = 207$, and as the computed elasticity amounts only to 23·8 atmospheres,

spheres, it follows that the formula does not reach such high temperatures.

If we examine the formula (B) it will readily appear that the quotient $\frac{\log. \frac{e}{30}}{t}$ decreases to a minimum and then increases again. And that this is actually the case in nature, may be proved by the experiments in our possession. Thus, taking the experiment of M. Clement, we have,

$$\frac{\log. 35}{207} = \cdot 007459;$$

but in the Table we find ·007454 in the column of quotients at 310°: wherefore while the temperature increased from 310° to 419°, the quotient must have decreased to a minimum and then increased again to its first magnitude. We learn, further, that the minimum takes place at 364°½, or about 152° or 153° beyond the boiling point. Now, by the formula, the minimum is 311° beyond the boiling point, or at double the distance it ought to be; and the experiment of M. Clement is placed before the minimum, instead of after it. The formula, therefore, although it is very accurate for a long range of temperatures, finally digresses altogether from the truth, furnishing another instance of the great difficulty of detecting general properties or laws by means of a comparison of particular results.

It is evident, however, that the formula deviates from the truth, not because the form of the expression has been erroneously assigned, but because the experiments do not enable us to determine the coefficients with sufficient accuracy. For this purpose it is requisite to know the exact relation between Δ and Δ^2, which we shall seek in vain to deduce from the quantities furnished by observation. We have in reality groped out the numerical values in the formula by considering the general features of the numbers, rather than by following any direct or accurate procedure. The experiment of M. Clement has shown us in what respects the formula is faulty; and, perhaps, it might be possible so to rectify it, as to make it represent all the experiments with some degree of approximation. This, however, could not be accomplished without long calculations, serving little other purpose than to gratify curiosity; for it cannot be supposed that a single experiment beyond the minimum is sufficient for fixing that point with any tolerable precision.

But, setting aside the consideration of numerical formulæ, it has been proved that the quotient of the elasticity divided by the temperature is a quantity that decreases to a minimum and

and then increases again. The general form of the expression has likewise been assigned; and it will readily appear that the quotient is represented by the square of the ordinate to the conjugate axis of a hyperbola, the square of the semitransverse axis being the minimum. To show this we need only put the expression (B) in this form, viz.

$$\frac{\log. \frac{c}{30}}{t} = A + B(n - t)^2,$$

A and B being known numbers, and n the distance of the minimum from the boiling point. But it is sufficient to have mentioned these things, the length of this paper warning me that it is time to stop writing.

Nov. 5, 1826. J. IVORY.

II. *On the Identity of Epistilbite and Heulandite. By* A. LEVY, *Esq. M.A. F.G.S.**

DR. GUSTAVUS ROSE, of Berlin, in the eighth number of the Edinburgh Journal of Science, has given the description of a mineral species which he has considered as new, and named Epistilbite. He states that the most considerable differences between this species and Heulandite are those in their regular forms, the physical characters and chemical compositions of both species being nearly exactly the same†. When I read his paper it struck me, however, that the forms and angles of Epistilbite might be derived by simple and frequently occurring decrements from the primitive form I had adopted for Heulandite, and that the easy, nacreous, and only cleavage of Epistilbite was parallel to the same modification of this primitive form, as is the easy, nacreous, and only cleavage of Heulandite. It appears to me, therefore, that if this mode of viewing the crystals of the newly described substance should be found correct, there is no sufficient ground to consider it as forming a distinct species from Heulandite; since the forms of these two substances, which had been thought to constitute their principal difference, may easily be made to agree.

* Communicated by the Author.

†

		Heulandite.	*Epistilbite.*
Specific gravity		2·211	2·249
Hardness			A little greater than Heulandite.
Blowpipe			The same indications as in Heulandite.
Analysis	Silica	59·95	58·59
	Alumina	16·87	17·52
	Lime	7·19	7·56
	Water	15·10	14·48
	Soda		1·78

Fig.

Fig. 1.

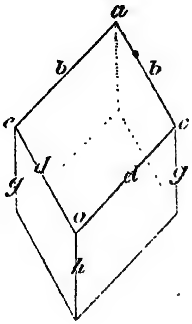


Fig. 2.

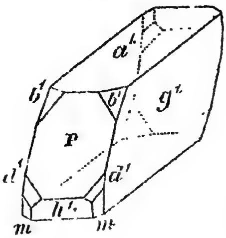


Fig. 3.

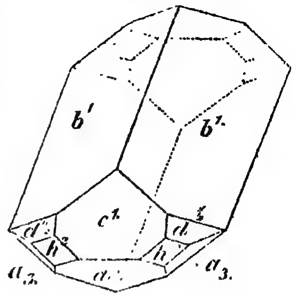


Fig. 4.

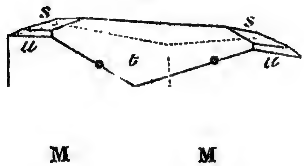


Fig. 5.

Fig. 4. represents the form of the crystals of Epistilbite, and is a copy of the figure given in the Edinburgh Journal of Science. The cleavage is parallel to a plane bisecting the obtuse angle formed by the two planes M. The incidences of the different planes of this figure given by Dr. Gustavus Rose are the following:

M on M	=	135°	10′
M on *t*	=	122	9
t on *t*	=	109	46
t on *u*	=	154	51
s on *s*	=	147	40

The primitive form I have adopted for Heulandite, in the description of Mr. Turner's collection, is an oblique rhombic prism represented fig. 1; the angles and dimensions of which are,

$$m, m = 97° 39' \quad P, m = 108° 1' \quad b : h :: 1000 : 588$$

the plane angle of the base is 92° 22′ 40″, and that of the lateral faces 106° 24′ 30″.

The

The ordinary form of the crystals of Heulandite drawn in parallel position with fig. 1, is represented by fig. 2, each plane bearing the sign which indicates the decrement by which it is conceived to be derived of the primitive. The following are the measurements of the angles.

P, h^1 = 114° 5′	m, h^1 = 138° 49′ 30″	m, g^1 = 131° 10′ 30″
P, a^1 = 130° 19′ 50″	a^1, h^1 = 64° 24′ 40″	a^1, b^1 = 147° 22′
P, b^1 = 147° 8′	m, b^1 = 75° 9′	b^1, b^1 = 135° 52′
	b^1, g^1 = 112° 4′.	
P, d^1 = 155° 25′ 10″	m, d^1 = 132° 36′	d^1, d^1 = 146° 31′ 20″
	d^1, g^1 = 106° 44′.	

The dimensions assigned to the primitive form of Heulandite, which have been determined so as to make the results of calculation agree as nearly as possible with those of observation, are such, that the line joining the angle o, (fig. 1.) with its opposite, does not make a right angle with the edge h. It is well known, that Haüy had erroneously supposed that in every primitive oblique rhombic prism this angle was right. In the present case, however, as in many others, this angle differs but very little from 90°, being equal to 89° 5′. If the property here alluded to, and assumed by Haüy, did really exist, it would result from it, that every modification produced by simple or intermediary decrement on the edges or angles of an oblique rhombic prism might be supposed to be composed of half the number of planes of a modification derived by some decrement on the edges or angles of a right rhombic prism; and consequently that to every such modification there would correspond another produced by a different decrement, the planes of which would be situated with respect to one pair of the lateral faces of the primitive precisely as those of the first were with respect to the other pair; the planes of one modification measuring with each other, or with the lateral faces of the primitive, exactly the same angles as the planes of the other. It is easy to see that if

$$\left(d^{\frac{1}{x}} \quad d^{\frac{1}{y}} \quad h^{\frac{1}{z}}\right)$$

represents generally the sign of one of the modifications, the sign of the other will be

$$\left(b^{\frac{1}{x+z}} \quad b^{\frac{1}{y+z}} \quad h^{\frac{1}{z}}\right)$$

And without entering into any discussion with respect to the results offered by these formulæ in particular cases, I shall only

only observe that the two modifications which they represent will coincide when

$$z = 0, \text{ or } x + y + z = 0.$$

A crystal derived from such an oblique rhombic prism, and composed only of planes belonging to pairs of such modifications, would appear therefore more easily derivable from a right rhombic prism, than from an oblique one.

If we consider now an oblique rhombic prism in which the line joining the angle o with its opposite is not perpendicular to the edge h, but very nearly so, the planes of the modification $\left(d^{\frac{1}{x}}\ d^{\frac{1}{y}}\ h^{\frac{1}{z}}\right)$ will not then measure with each other, or with the lateral faces of the primitive exactly the same angles, as the planes of the modification $\left(b^{\frac{1}{x+z}}\ b^{\frac{1}{y+z}}\ h^{\frac{1}{z}}\right)$, but, however, the differences between the corresponding incidences of these two modifications will be very small. Consequently a crystal derived from such an oblique rhombic prism, and composed only of pairs of such modifications, would also appear more easily derivable from a right rhombic prism than from an oblique one, and especially if the measures of angles obtained by observation could not be sufficiently depended upon, owing to the want of brilliancy of the faces, or the imperfection of the crystal, to establish any difference between the incidences of the planes of one modification, and the incidences of the planes of the corresponding one.

It is doubtful whether among the substances, whose crystals may be derived from an oblique rhombic prism, there is any, in which the property assumed by Haüy does really exist. But it is certain that there are many in which it very nearly obtains; and it is not less certain, and perhaps still more remarkable, that in many of these, the pairs of modifications whose general signs are $\left(d^{\frac{1}{x}}\ d^{\frac{1}{y}}\ h^{\frac{1}{z}}\right)$ and $\left(b^{\frac{1}{x+z}}\ b^{\frac{1}{y+z}}\ h^{\frac{1}{z}}\right)$ occur often on the same crystal. Pyroxene and amphibole present several instances of what has just been stated.

This being well understood, it will not be difficult to show in what manner the crystals of Epistilbite may be derived from the primitive form of Heulandite. The symmetry of fig. 4. which represents the form of these crystals, suggests at first the idea that they are derived from a right rhombic prism, but from the above considerations, does not exclude the possibility of their being derived from the primitive form

of Heulandite; since in that form the line joining the angle o with its opposite, makes with the edge h an angle of 89° 5′, and also since the faces of the crystals, according to Dr. Rose's statement, are most of them uneven and dull, and do not admit of measurement by the reflective goniometer. If we now compare the measures of the angles of the two substances, we find in Epistilbite M on M (fig. 4) = 135° 10′, and in Heulandite b^1 on b^1 = 135° 52′. The cleavage of the first substance being parallel to a plane, bisecting the angle formed by the planes M, and the cleavage in the second being parallel to a plane bisecting the angle formed by the planes b^1. The faces M (fig. 4) may therefore be considered as corresponding to the planes of the modification b^1 of Heulandite. The difference of 42′ between the two angles which we suppose thus to be equal, may very well be accounted for by the imperfection of the crystals of Epistilbite. But I may even add, that having found in Sir Abraham Hume's collection a specimen from Faroe, covered with crystals, answering perfectly the description given of Epistilbite, I was allowed to detach a few of them, and by cleaving them deeply so as to leave only a very narrow part of the planes M, I could measure the incidence of M on M by means of the reflective goniometer, and found it constantly from 5 to 10 minutes less than 136°. It still remains to show the correspondence of the other planes s, t, u (fig. 4) with some simple modification of Heulandite. This is the object of the following comparative table; and the near agreement between the angles of the planes of Epistilbite, and the calculated angles of the modifications of Heulandite, prove I believe satisfactorily, that the planes s, (fig. 4) may be considered as the result of a decrement by two rows along the lateral edges h of the primitive of Heulandite; two of the faces t as the result of a decrement by one row on the angle o; and the two others of a decrement by two rows in height on the angle a; two of the faces u as the result of a decrement by two rows in height on the edges d; and the two others as the result of a decrement by three rows in breadth on the lateral angles a.

Calculated angles of the modifications of Heulandite.	Angles of Epistilbite according to Dr. Rose.
$h^2, h^2 = 147° 40' 40''$	$s, s = 147° 40'$
$b^1, b^1 = 135° 52'$	$M, M = 135° 10'$
$o^1, b^1 = 122° 15'$ $a^{\frac{1}{2}}, b^1 = 123° 19'$	$t, M = 122° 9'$
$o^1, a^{\frac{1}{2}} = 108° 29'$	$t, t = 109° 46'$
$d^{\frac{1}{2}}, o^1 = 154° 31'$ $a_3, a^{\frac{1}{2}} = 154° 11'$	$t, u = 154° 51'$

Fig.

Fig. 3. represents the same form as fig. 4, but drawn in a parallel position with fig. 1, and with regard to the decrements by which the planes are conceived to be derived from it.

It appears to me, therefore, that the crystals of Epistilbite present only a simple variety of Heulandite, the crystallographical sign of which referred to the primitive form assumed for that substance is

$$h^2 \; a^{\frac{1}{2}} \; a_3 \; o^1 \; b^1 \; d^{\frac{1}{2}}.$$

The difference of their general appearance with that of the ordinary crystals of Heulandite, can be no reasonable objection to their re-union with that substance; since it is well known that some of the varieties of sphene differ quite as much in their forms.

The crystals of Epistilbite are generally found in twins and joined parallel to one of the planes M (fig. 4), or b^1 (fig. 3). This circumstance may induce to take for the primitive form of Heulandite an oblique rhombic prism, the lateral planes of which would be the planes now designated by b^1, and for its base either of the planes P, h^1, a^1, o^1 or $a^{\frac{1}{2}}$.

It might be objected to the reasons given above to consider Epistilbite as a simple variety of Heulandite, that similar arguments might perhaps be used to show that Stilbite may also be considered as a variety of the same species. The hardness and specific gravity of Heulandite and Stilbite are nearly the same; they have the same cleavage, even their chemical compositions do not widely differ; and though the forms of Stilbite are generally referred to a right rhombic prism, they might still, as in the case of Epistilbite, be derived from the primitive of Heulandite. If that were really possible, it would undoubtedly tend to throw some doubt upon the accuracy of the reasoning used with respect to Epistilbite; because, besides the difference between the chemical compositions of Heulandite and Stilbite, and that of their optical characters, there are still other reasons to regard them as two distinct species.

But if, on the contrary, it is shown that, consistently with the simplicity of the structure of crystals, it is impossible to refer both the forms of Stilbite and Heulandite to the same primitive form, it will be a strong argument in favour of the union of Epistilbite with the last-mentioned substance.

The form of the crystals of Stilbite is represented by fig. 5; and the mean of various measurements gives 119° 5′ for the most obtuse incidence of the faces of the pyramid formed by the planes marked b^1, and 114° for the other. Then, by assuming the primitive form to be a right rhombic prism of such dimensions that the planes b^1 should result of a decrement by one row on the edges of its base, it follows that the incidence

of

of the lateral planes of this prism is 94° 6′, and also that the angle of the two oblique edges of the pyramid b^1, which are in a plane parallel to the modification h^1 is equal to 105° 37′. Now considering that the plane of cleavage of Stilbite, which is the face g^1 of fig. 5, ought to be in position parallel to the plane of cleavage of Heulandite, there are only two ways in which it may be tried to derive the form fig. 5. from the primitive adopted for Heulandite. The first is when the crystal of Stilbite is placed, with respect to the primitive of Heulandite, in the relative position represented by fig. 1. and 5; that is to say, when the intersection of h^1 and g^1 (fig. 5) is parallel to the lateral edge of fig. 1. The second, when the crystal of Stilbite is placed so, that the line of intersection of the planes h^1, g^1 (fig. 5) is parallel to the intersection of b^1 and g^1 (fig. 2). But in both cases it will be found that the faces b^1 could only be derived from fig. 1. by complicated intermediary decrements. They might be made to correspond in the first case to the planes of the two modifications the crystallographical signs of which are

$$\left(d^1\, b^{\frac{2}{3}}\, g^{\frac{5}{9}}\right), \qquad \left(b^{\frac{1}{4}}\, b^{\frac{2}{13}}\, h^{\frac{1}{9}}\right)$$

and in the second to the planes of the modifications whose signs are

$$\left(d^1\, b^{\frac{2}{13}}\, g^{\frac{6}{19}}\right), \qquad \left(b^{\frac{1}{29}}\, b^{\frac{2}{13}}\, h^{\frac{6}{19}}\right).$$

And even then the angles calculated from these indices would not exactly correspond to the angles measured; and to obtain a nearer equality between the results of observation and calculation, higher numbers still should be used for the indices. There can remain therefore no doubt respecting the incompatibility of the forms of Stilbite and Heulandite.

III. *Reply to Mr.* Galbraith's *Remarks on the Experiments for ascertaining the Velocity of Sound at Port Bowen. By Capt.* W. E. Parry, *R.N. F.R.S., and Lieut.* H. Foster, *R.N. F.R.S.*

To Richard Taylor, Esq.

Dear Sir, H. M. S. Hecla, Deptford, Dec. 11, 1826.

IN your valuable Magazine*, No. 341, is a communication from Mr. Galbraith of Edinburgh, making some remarks on our experiments for ascertaining the Velocity of Sound at Port Bowen; to which we should have replied earlier, had not circumstances connected with our present engagements interfered at the time of our becoming acquainted with the remarks in question.

In the first place, Mr. Galbraith *seems to regret* that in such

* Phil. Mag. vol. lxviii. p. 214.

a climate

a climate we should not have recorded more particularly the several circumstances which are required in such experiments. It does not appear, however, that we have neglected any circumstance which he has pointed out as necessary, except the *precise* velocity of the wind,—for which purpose unfortunately we had no anemometer: nevertheless the velocity of the wind during our experiments must, we conceive, be of little importance; as in all cases, save two, the observations were made in calm weather, or when very light airs prevailed, as stated in the table. With respect to "the angle between the direction of the sound and that of the wind," we beg to observe that, as the exact bearing of the gun is given, and likewise the direction of the wind to the nearest point, we do not comprehend what reason Mr. Galbraith has for regretting that "the direction of the wind relative to that of the sound was not ascertained."

The circumstance of our not having noted the state of the hygrometer, cannot properly be considered an omission, since its indications were always those of perfect dryness in very low temperatures as stated in the body of the narrative.

It is not therefore to the omission of the velocity of the wind, that the want of coincidence between Mr. Galbraith's *theoretical deductions*, and our practical results, is to be attributed; nor is the mistake we have fallen into ascribable to the same cause, but solely from our having inadvertently spoken of an increased density, where we ought to have said a diminished elasticity. At the same time we think Mr. Galbraith has by no means *fairly* stated our remark, having omitted that part which applies to temperature: for we did not speak of the increased density of the atmosphere, unconnected with temperature; whereas, as Mr. Galbraith has quoted our remark, it appears that density as measured by the barometer alone was contemplated. However, Mr. Galbraith will perhaps do us the justice to believe, that it was certainly far from our intention to oppose our opinions on these points to those of Newton or Laplace. We considered our remark at the time, as a fair deduction from our own experiments, without at all considering with what theory it might be at variance: our only wish being, to furnish data for philosophers to arrive at such laws as will make the computed and observed velocities of sound agree more exactly with each other, than appears to be the case, in the present state of our information of all the modifying circumstances to which the motion of sound is subjected.

We are, Sir, yours, &c.

WILL. EDW. PARRY, Capt. } R. N.
HENRY FOSTER, Lieut. }

IV. *Ex-*

IV. *Experiments on the Cohesion of Cast-Iron.* By B. BEVAN, Esq.

To Richard Taylor, Esq.

Sir,

AN erratum, of a single figure in the third formula in the last Number of the Philosophical Magazine, page 343, may have a tendency to mislead some of your readers. It will be noticed that the third as printed, is the same as the second, but should have been $\frac{3\,l\,w}{b\,d^2} = c$.

Experiments on the *cohesion* of *cast-iron* lead to very irregular results, depending in some degree upon the quality of the metal; but in a greater degree upon the improper mode of applying the force. During the present year I had some cylinders and prisms cast of gray soft metal; I then reduced the middle part of one of the cylinders to ·425 inches diameter and fixed it in my experimental press, with the resultant of the straining force, nearly in the axis of the cylinder, the weight required to break it was 2550 pounds; at the place of fracture was a visible *fault in the casting*, so that the proper strength must be more than 17,900 pounds to the square inch.

I then took another cylinder and reduced it, by turning, to exactly ·5 or $\frac{1}{2}$ an inch in diameter, and submitted it to the like process. The breaking weight was 6430 pounds, showing a strength of cohesion of 32,700 pounds per square inch.

This last specimen sustained a load of 5988 pounds for five minutes, before the last motion or addition of the weight: so that we cannot be wrong in estimating iron of this quality to have a cohesion of more than 30,000 pounds to the square inch. The specific gravity of the iron was 7·716. I also tried the *transverse strength* of some of the same iron cast into prismatic bars: these bars were placed horizontally upon two supports, twelve inches asunder, and the weight applied on the middle of the bar; the depth being ·65, and breadth ·49 inches respectively. The gradual application of the load occupied three hours; and this bar sustained a load of 700 pounds for ten minutes without signs of fracture: an addition of 10 pounds broke it. If we therefore take the formula $\frac{1{\cdot}5\,l\,w}{b\,d^2} = c$, the cohesion appears equal to 61,000 pounds to the square inch.?

Another bar of the same metal, and of nearly the same dimensions, was tried with the least thickness vertical, *i. e.* in the manner of using it, the depth and breadth being ·487 and ·64, having had a part of the hardened surface removed by a fine file.

Being

Being partly aware of the transverse strength from the former experiment, the time used for applying the first part of the load was shortened; but towards the conclusion the additions were cautious and slow, and the whole time occupied in the experiment was $57\frac{1}{4}$ minutes; the breaking weight was 506 pounds, which gives 59,950 pounds per square inch, for the cohesion, or in round numbers 60,000 pounds, and the mean of both 60,500 pounds. Yours truly,

Leighton, Dec. 11, 1826. B. BEVAN.

P.S. I have lately seen in the public papers, under the title of *Curious Weather-gauge*, an account of a machine invented by Mr. Donovan, capable of showing the commencement and termination of every shower, and the rate of raining, with the quantity fallen.

I beg leave to say, that I have several rain-gauges of such a nature as to exhibit all these particulars; one of which has been in constant use for fifteen years.—Further particulars I perhaps may give at another time.

V. *Table and Formulæ for reducing Registers of the Height of the Barometer to the Standard Temperature and Level. By Mr.* J. NIXON.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

ONE of the many obstacles to the advancement of the degraded science of Meteorology, is confessedly the difficulty of an immediate comparison of the pressure of the atmosphere, as registered at places differing in elevation and temperature. So intolerably irksome to the man of genius, anxious to devote his talents to this obscure branch of philosophy, must be the unintellectual toil of rendering a vast number of similar registers comparable with each other, that the true lover of the science cannot but indulge in the wish, that the editors of scientific journals would invariably decline the insertion of meteorological registers unreduced to the standard level and temperature.

To render the task thus imposed on the observer as easy and brief as is compatible with the requisite accuracy, I have taken the liberty of transmitting you the annexed Table, derived from the following formulæ: in which h denotes the observed height of the barometer corrected for the varying height of the surface of the mercury in the cistern, and for the effect of capillary attraction; T the height of the detached thermometer (or temperature of the mercury); a the elevation of

of the barometer in *feet* above the level of the sea; and t the observed temperature of the air.

Formula No. 1.—Reduction of the pressure h to its value at the standard temperature of 32° Fahr. $= h'$.

$$h' = h \pm \frac{\overline{T \backsim 32} \times h}{11121 + T};$$

additive or subtractive as T is below or above 32° Fahr.

No. 2.—Reduction of the pressure at 32° Fahr. $= h'$ to its value at the level of the sea $=$ H.

$$\log. H = \log. h' + \frac{a}{56054.\left(1 + \frac{t + \frac{a}{500}}{418}\right)}$$

No. 3.—Reduction of the observed pressure h to its value at the level of the sea, and standard temperature of 32° Fahr. $=$H.

$$\log. H = \log. h \pm \frac{\overline{69{\cdot}8\left(1 + \frac{t + \frac{a}{500}}{418}\right) + a} \backsim \overline{2{\cdot}2\left(1 + \frac{t + \frac{a}{500}}{418}\right) \times T}}{56054.\left(1 + \frac{t + \frac{a}{500}}{418}\right)}:$$

the upper or lower sign to be used as 69·8 &c. $+ a$ is greater or less than 2·2 &c. $\times$ T *.

Explanation of the Use of the Table.

For formula No. 3.—1°. To the observed temp. of the air *add* $\frac{1}{300}$ part of the alt. which term the *mean* temp.

2°. With the *mean* temp. enter the col. A, and take out the corresponding quantity in col. B, which add to the altitude.

3°. With the *mean* temperature for the first column take out the corresponding quantity in column C, which *multiply* by the height of the detached thermometer.

4°. Note the *difference* of this *product* and the *sum* last found, marking it positive or negative as the *sum* is greater or less than the *product*.

5°. Multiply this *difference* by the quantity given in the column D corresponding to the *mean* temperature found in the first column, and divide the product by 1000.

6°. To the logarithm of the observed pressure, (corrected for capacity and capillarity,) add or subtract, according to the sign prefixed to the *difference*, the quotient just found, which

* Approximatively, $$\log. H = \log. h \pm \frac{\overline{a + 78} \backsim \overline{2{\cdot}4 \times T}}{56054.\left(1 + \frac{t + \frac{a}{500}}{418}\right)}.$$

will

will be the logarithm of the pressure required, at the standard temperature of 32° Fahr.—Example:

$h = 29{\cdot}500$ inches; $T = 60°$; $t = 59°$; $a = 500$ feet.

$$\frac{1}{500})\underline{500} \qquad\qquad 79{\cdot}9 \text{ (See column B.)}$$
$$\underline{1} \qquad\qquad +\ \underline{500{\cdot}0}$$
$$+\ 59 \qquad\qquad 579{\cdot}9 \text{ Sum}$$
$$\text{mean temp. } \overline{60}° \qquad 2{\cdot}5 \times 60 = \underline{150{\cdot}0} \text{ Product}$$
$$+\ 429{\cdot}9 \text{ Difference}$$
$$\times\ \underline{{\cdot}015601} \text{ (See column C.)}$$
$$\frac{1}{1000})\underline{6{\cdot}7069}$$
$$+\ {\cdot}0067069$$
$$\text{log. of } 29{\cdot}500 = \underline{1{\cdot}4698220}$$
$$\text{log. of } 29{\cdot}959 = \underline{\underline{1{\cdot}4765289}}$$

For formulæ Nos. 1 and 2.—If we prefer to reduce the observed pressure in the first instance to its value at 32° Fahr. the process of calculation will be as follows:

$$\text{By Formula No. 1. } \frac{60 - 32 \times 29{\cdot}5}{11121 + 60} = \frac{826}{11181} = -\ {\cdot}0739$$
$$h = \underline{29{\cdot}5000}$$
$$h' = \underline{\underline{29{\cdot}4261}}$$

Entering the first column of the table with the mean temperature, take the corresponding number in the last column, which multiply by the altitude. Divide the product by 1000, and add the quotient to the logarithm of the pressure at 32° Fahr.—Example:

$${\cdot}015601$$
$$\times\ \underline{500}$$
$$\frac{1}{1000})\underline{7{\cdot}8005}$$
$$+\ 0{\cdot}0078005$$
$$\text{log. of } 29{\cdot}4261 = \underline{1{\cdot}4687327}$$
$$\text{log. of } 29{\cdot}959 = \underline{\underline{1{\cdot}4765332}}$$

The elevation of the barometer, when placed in the vicinity of the sea, may be readily ascertained by levelling, trigonometrically, or by the portable barometer. When the situation is inland, its height relative to the nearest canal, mountain, or other object of which the absolute elevation has been well determined, may be obtained by one or other of the methods above mentioned. As a last resource,—extract from the different scientific journals the annual *mean* pressures furnished by barometers placed at *given* elevations above the sea in the vicinity of the observer, which data, in addition to the annual *mean* height of his own instrument, will enable him to ascertain on calculation the required difference of level.

I have the honour to be, &c.

Leeds, Dec. 5, 1826. J. Nixon.

TABLE.

A	B	C	D	A	B	C	D
3° F.	70·3	2·2	0·017713	43° F.	77·0	2·4	0·016176
4	70·5	2·2	·017671	44	77·2	2·4	·016141
5	70·7	2·2	·017629	45	77·4	2·4	·016106
6	70·8	2·2	·017587	46	77·5	2·4	·016071
7	71·0	2·2	·017546	47	77·7	2·4	·016037
8	71·2	2·2	·017505	48	77·9	2·4	·016002
9	71·3	2·2	·017464	49	78·0	2·4	·015968
10	71·5	2·2	·017423	50	78·2	2·4	·015934
11	71·7	2·2	·017383	51	78·4	2·4	·015900
12	71·8	2·2	·017342	52	78·5	2·5	·015866
13	72·0	2·3	·017302	53	78·7	2·5	·015832
14	72·2	2·3	·017262	54	78·9	2·5	·015799
15	72·3	2·3	·017222	55	79·0	2·5	·015765
16	72·5	2·3	·017182	56	79·2	2·5	·015732
17	72·7	2·3	·017143	57	79·4	2·5	·015699
18	72·8	2·3	·017103	58	79·5	2·5	·015666
19	73·0	2·3	·017064	59	79·7	2·5	·015633
20	73·2	2·3	·017025	60	79·9	2·5	·015601
21	73·4	2·3	·016987	61	80·0	2·5	·015568
22	73·5	2·3	·016948	62	80·2	2·5	·015536
23	73·7	2·3	·016910	63	80·4	2·5	·015503
24	73·9	2·3	.016871	64	80·5	2·5	·015471
25	74·0	2·3	·016833	65	80·7	2·5	·015439
26	74·2	2·3	·016795	66	80·9	2·5	·015407
27	74·4	2·3	·016758	67	81·0	2·5	·015375
28	74·5	2·3	·016720	68	81·2	2·5	·015344
29	74·7	2·3	·016683	69	81·4	2·5	·015312
30	74·9	2·3	·016645	70	81·5	2·5	·015281
31	75·0	2·3	·016609	71	81·7	2·6	·015250
32	75·2	2·3	·016571	72	81·9	2·6	·015219
33	75·4	2·4	·016535	73	82·0	2·6	·015187
34	75·5	2·4	·016498	74	82·2	2·6	·015157
35	75·7	2·4	·016462	75	82·4	2·6	·015126
36	75·9	2·4	·016425	76	82·5	2·6	·015095
37	76·0	2·4	·016389	77	82·7	2·6	·015065
38	76·2	2·4	·016353	78	82·9	2·6	·015034
39	76·4	2·4	·016318	79	83·0	2·6	·015004
40	76·5	2·4	·016282	80	83·2	2·6	·014974
41	76·7	2·4	·016246	81	83·4	2·6	·014944
42	76·9	2·4	·016211	82	83·5	2·6	·014915

VI. *Further List of Errors in* Piazzi's *Catalogue of Stars.*

IN vol. lxvi. p. 261, of the Philosophical Magazine, a list of certain errata in Piazzi's celebrated Catalogue of stars was given, which had either been pointed out by himself, or discovered by subsequent observations. We are indebted to the same correspondent for the communication of the following list of *additional* errors, which he has discovered, on a careful comparison of the catalogue of Piazzi, with those of Flamsteed, Bradley and Mayer. These errors are principally in the *names of the stars*, and do not affect the general accuracy of that invaluable work.

Hora et Numerus.	Columna.	Errata.	Corrige.
I. 151	Nomen	54 φ Androm.	54 Androm. (φ Persei
210		28 *e* Cassiopeæ	48 Cass. (*e* Cus. Mes.
IV. 181		β Sculptoris	β Cœli Scalp.
278 &c.	Numerus	178 . 179 . 180	278 . 279 . 280
40	Nomen	40 τ Orionis	20 τ Orionis
76		27 ρ 2 Orionis	27 p Orionis
78		25 ψ 2 Orionis	25 ψ^1 Orionis
133		11 Leporis	10 Leporis
262		35 δ Aurigæ	33 δ Aurigæ
265		54 χ 4 Orionis	57 χ^2 Orionis
VI.		F 1 Orionis	f^1 Orionis
29		F 2 Orionis	f^2 Orionis
37		74 κ 2 Orionis	74 k^2 Orionis
205		Navis	Navis
247		36 D Geminorum	36 d Geminorum
253		X Navis	x Navis
266		38 E Geminorum	38 e Geminorum
VII. 3		48 M Geminorum	48 m Geminorum
21		52 N Geminorum	52 n Geminorum
45		27 E 1 Canis	27 e^1 Canis
59		Navis 618 C. A.	Canis 618 C. A.
69		56 Q Geminorum	56 q Geminorum
72		30 D Canis	30 d Canis
101		53 P Geminorum	63 p Geminorum
105		62 S Geminorum	2 ρ Geminorum
111		65 B 2 Geminorum	65 b^2 Geminorum
131		68 K Geminorum	68 k Geminorum
147		N 1 Navis	n^1 Navis
163		P Navis	p Navis
166		74 F Geminorum	74 f Geminorum
173		M Navis	m Navis
194		81 G Geminorum	81 g Geminorum

Hora et Numerus	Columna.	Errata.	Corrige.
VII. 214	Nomen	C Navis	c Navis
246	———	85 L Geminorum	85 l Geminorum
VIII. 77	———	29 Geminorum	29 Cancri
158	———	48 υ Cancri	48 ι Cancri
X. 118	———	1 φ 1 Hydræ	1 φ^1 Hydræ
227	———	62 g Leonis	62 p Leonis
XI. 62	———	15 χ Crateris	15 γ Hydræ et Crat.
XII. 92	———	μ Centauri	u Centauri
119	———	22 q Virginis	21 q Virginis
126	Motus Prop.	— 0″,02	— 1″,02
177	Ascen. Recta	189° 6′ 43″ 8	189° 9′ 43″ 8
XIII. 1	Nomen	ω Centauri	ϖ Centauri
XIV. 117	———	24 γ Bootis	27 γ Bootis
198	———	32 h 2 Bootis	38 h^2 Bootis
266	———	λ Centauri.	λ Lupi
XV. 135	———	6 ν Coronæ bor.	6 μ Coronæ bor.
XVI. 71	———	5 g Scorpii	5 g Ophiuchi
XVII. 286	———	51 ψ 1 Draconis	31 ψ^1 Draconis
XIX. 57	———	21 ω 1 Aquilæ	25 ω^1 Aquilæ
135	———	35 o Aquilæ	35 c Aquilæ
187	———	39 k Antinoi	39 $\varkappa$ Aquilæ
242	———	46 Aquilæ	47 Aquilæ
273	———	51 Aquilæ	Aquilæ
289	———	8 ξ Sagittæ	8 ζ Sagittæ
XX. 233	———	15 ν Capricorni	15 υ Capricorni
433	Hora	29.53	20.53
XXII. 111	Nomen	51 ζ Aquarii	55 ζ Aquarii

VII. *On the Inflammation of Gunpowder and other Substances by Electricity; with a Proposal to employ the Term* Momentum *as expressive of a certain Condition of the Electric Fluid.* By Mr. W. Sturgeon.

To the Editor of the Philosophical Magazine and Journal.

Sir,

SINCE the publication of my paper on the ignition of gunpowder by the electric fluid, I have had an opportunity of perusing Mr. Woodward's paper * on the same subject. But I find that, like all other authors I have yet read, Mr. W. has

* This paper is one of those you so obligingly pointed out to me for perusal; and appears at page 283. vol. vii. of the new series of the Annals of Philosophy. The two papers in vol. viii. contain nothing material on the subject.

has left us entirely in the dark with respect to the *law* that is necessary to be observed in varying the experiment. For although the dimensions of his jar and tube are given, no mention whatever is made, why it is necessary to observe those dimensions, or why other jars and tubes of various other dimensions may not answer as well. Indeed, so little does Mr. W.'s hypothesis concur with the conclusions deduced from my experiments, that he supposes the water derives its retarding property *solely* from its confinement in tubes. For it is stated in Mr. W.'s paper, "If the tube be filled with æther or alcohol, and placed in the circuit, the powder will be inflamed. If it be filled with sulphuric acid, which is a better conductor, the powder will be scattered and not inflamed: but the dispersion will not be so great as when metals only form the circuit."

"The same effect will be produced by transmitting the charge through the animal œconomy, or through water *not* inclosed in tubes, in which case the water does not appear to oppose a sufficient resistance to the passage of the fluid."

It is evident from this statement of Mr. Woodward's, that he never varied the experiment by employing tubes of various diameters; his experiments being with different fluids in the same tube. Had Mr. W. taken into consideration the necessity of varying the diameter of the tube with the nature of the charge, I suspect he would have found little difficulty in igniting gunpowder by transmitting the electric fluid through sulphuric acid, it being necessary to observe nothing more than to augment the charge, or reduce the diameter of the tube.

That the retarding property of water does not depend on its confinement, is evident from the success of my experiments with a moistened thread exposed to the open air. Indeed, so certain it is, that confinement does not impart to water this property, that gunpowder may be ignited in the electrical circuit, by transmitting the fluid through a sufficiently narrow streak of water, drawn on the surface of a piece of flat glass.

I should, however, be extremely sorry to detract any thing from the merits of any man; and although Mr. Woodward's explanation of igniting gunpowder by the electric fluid, when transmitted through water, does not exactly agree with the principles on which I suppose the action to depend; I nevertheless should be wanting in candour, were I not to express my sentiments on the highly interesting nature of his experiments: and I feel a pleasure in acknowledging that Mr. W. has preceded me in some that are detailed in my paper (although I did not know it at the time); viz. those wherein gun-

powder

powder was ignited on both the positive and negative side of a long conducting wire.

Mr. Leuthwaite has likewise conducted a number of interesting experiments on this subject; but I believe they were intended chiefly to ascertain the *conducting power* of different fluids, in order to make choice of the most eligible for the purpose of igniting gunpowder by the electric fluid.

Hence, so far it appears that the idea had not been entertained, (prior to the institution of my experiments,) that the diameter of the tube, and the nature of the charge, would any way affect the result of the experiment. Neither does it appear that the ignition of gunpowder, by means of transmitting the electric fluid through *unconfined* water, was ever attempted prior to those experiments detailed in my former paper on this subject. The most extraordinary method of effecting the ignition of gunpowder by the electric fluid, that I have yet heard of, is that stated by Mr. Howldy in the Philosophical Magazine, vol. lxviii. p. 173. I have been induced to pay some attention to this method, "*especially* as it saves the experimenter time, labour, and power," circumstances highly important and necessary to be understood.

I think, however, it is to be regretted, that Mr. Howldy has not mentioned the hygrometrical state of that part of the table ("four inches") between the extremity of the chain and the outside of the jar: as it is possible, that a variation in that particular may vary the result of the experiment. But as Mr. H. has practised this method for several years, and with uniform success, it is to be expected that such a circumstance could hardly escape the notice of so accurate an experimenter. Considering, however, that four inches is a long striking distance through dry air, and not happening to be successful when attempting to repeat the experiment according to Mr. H.'s directions, I have been induced to suggest to that gentleman, the necessity of his repeating the experiment, under the following circumstances.

First. Let the table on which the jar and chain are placed be perfectly dry, and of hard wood, (say an old mahogany table that has frequently been rubbed with bees-wax, or with any thing else to render it a nonconductor.) Let this jar of 160 square inches of (interior) coated surface, be charged to the intensity of 80° per quadrant electrometer; every other part of the circuit being as he has described it. Discharge the jar through this circuit.

Secondly. Let the same arrangement again be made, only with this difference: draw on the table a narrow line of water

(four

(four inches long,) from the outside of the jar to the extremity of the chain. Discharge the jar through this circuit.

Should there occur different results in those experiments, (as I am persuaded there will,) why did Mr. Howldy not mention such essential particulars? Or are we to conclude from his silence, that he entertained just notions of the nature of the experiment, and that this circumstance was too trivial to notice?

With respect to the method in which the wooden peg is used, I am aware that it will answer very well if regulated upon the principles stated in my paper:—that is, when the moisture it contains in a transverse section is proportioned to the charge transmitted; and that, whether the point be sharp or blunt; and in whatever part of the circuit it may be placed. If its being "very dry" were essential to the success of the experiment, it appears strange, that when it possessed that quality in a superior degree, by "two or three experiments," it should be "rendered useless." Its having a sharp point too, may possibly be the means of its possessing properties which I have not seen; especially as it appears to have been used in that shape by the celebrated electricians Dr. Watson and Mr. Wilson more than fifty years ago.

I should however recommend those persons who use a piece of wood, to have it somewhat longer than that described by Mr. Howldy, for fear of meeting similar disappointments to those which he met with when varying the distance between the outside of the jar and the extremity of the chain with high intensities; as it is possible that the peg may be shorter than the striking distance.

Whenever an electrical discharge is transmitted through water, for the purpose of igniting gunpowder, the length of the aqueous column ought always to exceed the striking distance; for if the wires which enter the extremities of the column are brought too near each other, the electric fluid will dart from one to the other with very little interruption and in all probability will scatter rather than ignite the gunpowder.

To insure success, the column or train of water, or any substance containing it, such as wood, twine, silk, paper, &c., should never be shorter than six inches; and the thickness or quantity of water contained in a transverse section must always be regulated according to the nature of the charge; that is, to the quantity and intensity of the electric fluid employed. When a small jar is used for this purpose, then the strip of water must be very thin, or narrow: if a larger jar be used, the

the thickness of the aqueous column may be increased. When the same jar is used with different intensities, then the lowest intensity requires the thinnest strip of water; not because the intensity is lower, but because the quantity of fluid is less: and although a column of water that answers for a low intensity will answer for every intensity that is higher; nevertheless, a column that would answer very well for a high intensity, might be far too large, to insure success, with an intensity that is very low.

As glass tubes are both convenient and elegant for regulating the diameter of a column of water; about seven or eight inches of barometer tube, $\frac{1}{10}$ of an inch diameter, answers the purpose very well, with any jar containing more than half a square foot of coated surface on each side; with any intensity of charge above 50° per quadrant electrometer*.

By employing a glass tube of the above dimensions filled with water, and a jar containing 120 square inches of coated surface on each side of the glass, gunpowder was ignited in the circuit at every trial, with any intensity above 20°. When two such jars were employed at the same time, gunpowder ignited with every intensity above 10°; sometimes with an intensity as low as 7° or 8°. The gunpowder was fine grained, and of the best quality. If the gunpowder is bruised to a fine powder, it will answer still better.

When moistened thread, twine, or wood is used, any of them may be cut in two, and separated a little, and the gunpowder will take fire at this interruption of the watery part of the circuit. Thus we learn that it is not necessary for metallic conductors to be in contact with the gunpowder, to insure its ignition by the electric fluid.

A chain may be made of alternate links of copper wire and tailor's thread, in such a manner as not to be easily distinguished from one that is all of metal. If such a chain be dipped in water, and made a part of the electrical circuit, gunpowder may be ignited in any part of that circuit, as though the thread were one continued piece.

When one of the above-mentioned jars was employed, and a copper conductor formed the circuit, æther was fired at an interruption, with every intensity above 20°. When the water tube formed a part of the circuit, æther could not be fired, though both jars were employed together, with an intensity of 90°.

* As quadrant electrometers do not afford uniform measures of electrical intensity, but differ from one another according to the weight of the ball, delicacy of suspension, &c. an intensity is here given, which, it is expected, will answer with the generality of them.

When

When four such jars were charged to the highest possible intensity, and discharged through the water tube,—gold leaf placed in the circuit was powerfully attracted, but not deflagrated.

One jar with an intensity of 60°, powerfully magnetized a sewing needle, placed in a spiral forming part of a complete metallic circuit. With two jars charged to the highest possible intensity, no magnetic effect was produced, when the tube of water formed a part of the circuit.

How do these experiments accord with the doctrine of *quantity* and *intensity?* In the galvanic process, a needle may be powerfully magnetized by a single pair of small copper and zinc plates; by which process, it is said, we have *quantity*, but not *intensity*. But by the experiments I have just now mentioned, it appears that *quantity* has not the power of magnetizing needles, without *intensity*. For when the electric fluid was not retarded by inferior conductors, a very small *quantity* produced the effect; but when the *intensity* of the discharge was reduced, although three times the former *quantity*, at least, was employed, not the slightest magnetic effect was produced. The truth is, we want another and a more appropriate term in electricity;—that term is *Momentum*. *Intensity* answers very well to express the relative degrees of concentration on the surface of jars, &c., but *momentum* is the proper term when the fluid is in motion. Hence then, although the less quantity of fluid produced on the needle an effect which the greater was unable to accomplish; this effect was probably owing to the *momentum* of the former exceeding that of the latter.

When tow or cotton is moistened with spirit of turpentine, or mixed with powdered resin, it will ignite by a very small electrical discharge when the whole circuit is of metal. But if the tube of water form a part of the circuit, the tow, or cotton, prepared as above, will not ignite, although two of the before-mentioned jars charged to the highest possible intensity be discharged through the circuit.

When a narrow slip of gold leaf was placed between two pieces of glass, and made to form a part of a complete metallic circuit; one jar with an intensity of 80° discharged through that circuit, completely exploded the gold leaf. When two jars were employed at the same time, and charged to a still higher intensity than the former, and the water tube entered into the circuit; a similar slip of gold leaf subjected to the discharge, remained uninjured.

When gold leaf and gunpowder were subjected at the same time to a discharge similar to the above, and the whole cir-

 cuit

cuit of metal; the former was completely exploded, but the latter substance was scattered only. When the water tube formed a part of the circuit, every other part arranged as before, the gunpowder ignited, but the gold leaf was undisturbed by the discharge of the jars.

Similar experiments were made with gunpowder and needles, gunpowder and æther, gunpowder and tow, prepared as above. When the circuit was completely metallic, the needles were magnetized, or the æther, the tow, &c. were fired: but the gunpowder was in no instance ignited. When the water tube formed a part of the circuit, the gunpowder was, in every case, ignited; but the other substances remained unaffected.

Hence we may conclude, that in order to magnetize pieces of steel, to explode metals, to ignite æther, or tow, with resin, &c. by electricity; *quantity* and *velocity*, or *momentum* of the fluid is required. But to ignite gunpowder, *quantity* and *time* are indispensable. That is, when the quantity is constant: to produce the former effects requires *velocity*, to produce the latter effect, *time*.

I remain Sir,
Your obedient servant,
W. STURGEON.

Artillery Place, Woolwich,
Nov. 24th, 1826.

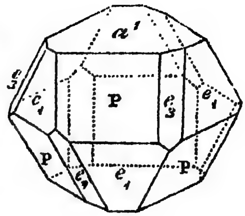
VIII. *On some newly discovered Siberian Minerals.* By A. LÉVY, *Esq., M.A. F.G.S.*

MR. MENGE has lately discovered in Siberia several rare species of minerals, which hitherto had not been found in that country, already so rich in that department of natural history. Among the newly discovered species are mentioned Tantalite, Gadolinite, and Zircon. However, from the characters of the specimens of these, which Mr. Heuland has just received and added to his private collection, it appears that the uncommonly well defined and detached crystals, which had been thought to belong to the first, present a form quite incompatible with those of that species, but agree perfectly with the description given by Professor Mohs of the substance he has called Axotomous iron ore, which is isomorphous with specular iron *. The form of the crystals from Siberia, in Mr. Heuland's collection, offer only two varieties; one of them is represented by fig. 1, and the other differs only from it by the absence of the faces marked e_3.

* I believe that the suggestion of Professor Mohs, that Crichtonite and Axotomous iron ore belong to the same species, will prove to be the truth; and I hope soon to be able to give the results of the comparative examination I have made of the two minerals.

This

Fig. 1.



This form, from the relative position of its planes, and the angles they measure, may be derived from an acute rhomboid, the planes of which would be the planes P of the figure, and measuring the same angle, or very nearly the same angle, as the primitive rhomboid of specular iron. The faces e_3, as shown by the figure, are not repeated symmetrically on each side of the faces P; so that there are only six of them, and they are disposed in such a manner that each of the three planes e_3 of the upper part of the crystal is parallel to one of the planes e_3 of the lower part. This regular want of symmetry is precisely the character offered by the crystals of axotomous iron ore. It is what Professor Mohs has expressed in saying that the regular forms of this substance are hemi-rhombohedral with parallel faces.

The crystals represented by fig. 1. are of a dark iron black colour; their planes are sufficiently brilliant for the use of the reflective goniometer, but however do not afford very good reflections. There are on some of them indices of cleavage in a direction perpendicular to the axis. They act upon the magnetic needle, but not so powerfully as specular iron. Their size vary from more than an inch in diameter to about one-fourth of an inch, their thickness is generally less than their breadth. Sometimes are found adherent to them small crystals or fragments of white felspar. Their exact locality, as well as that of the other minerals above mentioned, is the neighbourhood of Lac Ilmen, west of Miask, in the government of Ecatherinebourg in Siberia.

The specimen sent as Gadolinite presents a very large crystal said to belong to that species, placed on flesh-coloured cleavelandite, or perhaps rather labradorite, as one of the two faces of cleavage, inclined to each other at an angle of about 93° 30′, is striated parallel to its intersection with the other; a character which I believe belongs to the last-mentioned substance. The form of the crystal is that of an obtuse rhombic prism, the acute lateral edges of which are emarginated by narrow planes, without any distinct termination. The faces of the prism are rough, and small crystals of light brown zircon are disseminated on them as well as in the matrix. The crystal is so engaged, that the incidence of the faces cannot be measured even with the common goniometer. The fracture is of a deep black with a resinous lustre.

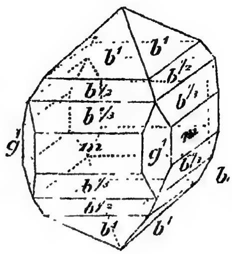
The zircons have come in crystals, detached and also disse-

 minated

minated on groups of milky white opaque felspar, most of the crystals of which are covered with a thin dark iron black coating, and mica in large laminæ. These crystals offer only two varieties; one of which is represented by fig. 2, and the other differs from it by the absence of the planes $b^{\frac{1}{2}}$ *.

They generally present both summits. The planes m and b^1 are very brilliant, the planes $b^{\frac{1}{2}}$ and $b^{\frac{1}{3}}$ dull, and the planes g^1 slightly undulated and of a highly vitreous lustre. Their colour varies from grayish white to a deep brown. Some are transparent; others only transluscent or opaque. Some of the crystals are as large as a wallnut, generally much smaller, but very well defined.

Fig. 2.



IX. *Observations on the Solar Eclipse, November 29th,* 1826. *By the Rev.* Baden Powell, *M.A. F.R.S.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

THE curious observations of Mr. Wiseman during the solar eclipse of September 1820, recorded in the Memoirs of the Astronomical Society (Part I. p. 140.) tend to show that during the eclipse there was a deficiency of the red rays of the sun and the heating power accompanying them. This conclusion is considered to be corroborated by the further observation of a diminution in the space occupied by the red rays in the prismatic spectrum formed at the same time. The facts are stated to have been anticipated by Mr. Wiseman; but the principle on which he anticipated them is not mentioned.

As there is nothing said in the paper alluded to, which can lead us to determine how far the effects may depend upon the *magnitude* of the eclipse, it will be doubtless a point of interest, not only on the occurrence of an eclipse of equal magnitude to verify the facts; but also in other cases to ascertain whether, or in what degree, similar effects are produced.

With this object in view, I made a few observations during the eclipse of November 29th. The weather was favourable only for one short interval in the early part of the eclipse,

* Since writing the above, I have observed another variety which besides the modifications of fig. 2. presents the planes, which Haüy has designated by x.

and

and again for a longer period towards its termination. My observations were made on the top of a house in the city:—unfavourable, however, as the circumstances were, I believe the results, as far as they go, can be relied on.

I adopted a somewhat different method from that used by Mr. W.; and one which appears to me capable of giving more accurate results. It consists in having two thermometers, the bulb of one painted red, the other black: these being fixed on one mounting with their bulbs detached, and exposed together to the influence of the sun's rays when eclipsed, and when in its ordinary state, any difference in the quantity of red rays and of their heating power, in the two cases, would be shown by a difference in the ratio of the risings of the two thermometers in a given time upon exposure to the sun. The thermometers were graduated to quarters of centigrade degrees, and the diameters of their bulbs were, red 0·6 inch; black 0·55 inch.

The following is a statement of the results I was enabled to obtain. The thermometers were held at a distance from surrounding objects, and were as much as possible screened from the wind, but not perfectly.

Nov. 29.

A.M. 10^h 25^m. Clouds cleared off. Eclipse considerable.

(I.)

Minutes.	Rise upon exposure to the Sun.		Ratio.
	Red therm.	Black therm.	red. : black.
1	centigr.	—	
2	0°·25	0°·5	1 : 2
...			
6	·5	1·	1 : 2
		Mean	1 : 2

10^h 45^m. Thick clouds.

11^h 20^m. Clouds cleared off. Eclipse considerable.

(II.)

1	·25	·5	1 : 2
2	·5	·75	1 : 1·5
3	·75	1·	1 : 1·3
		Mean	1 : 1·6

A.M. 11^h 40^m.

A.M. $11^h\ 40^m$. Eclipse diminished.

(III).

Minutes.	Rise upon exposure to the Sun.		Ratio.
	Red therm.	Black therm.	red. : black.
1	·25	·5	1 : 2
2	·5	1·25	1 : 2·3
3	·5	1·5	1 : 3
		Mean	1 : 2·4

$11^h\ 50^m$. Eclipse very small.

(IV.)

1	·5	1·25	1 : 2·5
2	1·	2·	1 : 2
		Mean	1 : 2·2

$12^h\ 5^m$. After eclipse ended.

(V.)

1	·25	·5	1 : 2
2	·5	·75	1 : 1·5
3	1·	1·5	1 : 1·5
		Mean	1 : 1·6

The results of these different observations exhibit some discrepancy, which may probably be attributed to the varying influence of wind, occasional haziness, &c.; but if those obtained during the eclipse be compared with those when there was none, I conceive no such difference will appear as can be supposed connected with the circumstance of the eclipse.

From these results we may infer, that *during the present eclipse*, (viz. one of about 6½ digits,) the rays underwent no such modification as was sufficient to produce a perceptible difference in the ratio of the effects on a red and a black thermometer.

From Mr. Wiseman's statement respecting the alteration which took place in the spectrum, I conceive it will be desirable to verify by the more perfect methods now known, of insulating homogeneous rays, whether in such an eclipse any given ray is either entirely wanting, or very much weakened.

So

So far as the case of the present eclipse bears upon the question, I tried this with respect to the extreme red rays discovered by Mr. Herschel, to which the greatest heating power belongs; and looking at the sun during the eclipse through a good prism of flint glass, interposing before the eye three thicknesses of the purple glass, described by that gentleman, I saw the ray in question perfectly distinct, and unaltered as compared with its appearance when there was no eclipse.

The deep red image of the sun thus forming the end of the spectrum, of course exhibited the eclipse; and in a spectrum formed in a darkened room, if the superposition was but small, the same deficiency at the red end might be apparent; but this obviously would not explain a deficiency of heat as compared with that of the compound rays; nor would it account for the increased brilliancy which Mr. Wiseman observed in the yellow and blue.

I am not aware what was the cause reasoned upon by Mr. W. as capable of producing any actual relative deficiency of red rays: if it were any such cause as inflexion of the sun's light in passing the body of the moon, which should affect the red rays most, and thus the light reaching the earth should be deprived of a portion of red, a less portion of green, blue, &c. this would produce a diminution in intensity, though not a deficiency in space, in the red part of the spectrum. Hence, however, would result a greater relative brightness in the blue, &c.; and such a difference in the heating effects as has been described. Such a cause would act more sensibly in porportion to the magnitude of the eclipse; and its effects might be quite insensible, except in a very considerable eclipse.

X. *The Bakerian Lecture. On the Relations of Electrical and Chemical Changes. By Sir* HUMPHRY DAVY, *Bart. Pres. R.S.**

I. *Introduction.*

A LONG time has elapsed since I read before this Society the Bakerian Lecture on the Chemical Agencies of Electricity. The general laws of decomposition developed in that paper were immediately illustrated by some practical results, which the Society did me the honour to receive in a very favourable manner; and which, by offering a class of new and powerful agents, led me away for many years into a field of

* From the Philosophical Transactions for 1826. Part III.

pure

pure chemical inquiry: and it is only lately, and on an occasion which is well known, that I have again taken up the subject of the general principles of electro-chemical action. After a number of new experiments, which I shall have the pleasure of laying before the Society, and notwithstanding the various novel views which have been brought forward in this and in other countries, and the great activity and extension of science, it is peculiarly satisfactory to me to find that I have nothing to alter in the fundamental theory laid down in my original communication; and which, after a lapse of twenty years, has continued, as it was in the beginning, the guide and foundation of all my researches.

I am the more inclined to bring forward these new labours at the present moment, though they are far from being in a finished state, because the discovery of Oersted and that of Morichini, illustrated by some late ingenious inquiries, connect the electro-chemical changes with entirely new classes of facts, and induce a hope that many of the complicated phænomena of corpuscular changes, now obscure, will ultimately be found to depend upon the same causes, and to be governed by the same laws; and that the simplicity of our scientific arrangements will increase with every advance in the true knowledge of nature.

II. *Some historical details.*

As I am not acquainted with any work in which full and accurate statements on the origin and progress of electro-chemical science are to be found, and as some very erroneous statements have been published abroad, and repeated in this country, I shall take the liberty of laying before the Society a short historical sketch on this subject; which is the more wanted, as the journal in which the early discoveries were registered has long been discontinued, and is now little known or referred to.

As there are historians of chemistry and astronomy who date the origin of these sciences from antediluvian times, so there are not wanting persons who imagine the origin of electro-chemical science before the discovery of the pile of Volta; and Ritter and Winterl have been quoted* amongst other persons as having imagined, or anticipated the relation between electrical powers and chemical affinities, before the period of this great invention. But whoever will read with attention Ritter's "Evidence that the galvanic action exists in organized Nature†," and Winterl's "*Prolusiones ad Chemiam Sæculi decimi noni*," will find nothing to justify this opinion.

* Oersted, translated by Marcel, 1813.

† Jena, 1800.

opinion. Ritter's work contains some very ingenious and original experiments on the formation and powers of single galvanic circles; and Winterl's some bold, though loose speculative views* upon the primary causes of chemical phænomena: and in the obscurity of the language and metaphysics of both these gentlemen, it is difficult to say what may not be found. In the ingenious, though wild views, and often inexact experiments of Ritter, there are more hints which may be considered as applying to electro-magnetism than to electro-chemistry; and Winterl's miraculous *Andronia* might, with as much propriety, be considered as a type of all the chemical substances that have been since discovered, as his view of the antagonist powers, the acid and basic, can be regarded as an anticipation of the electro-chemical theory. The queries of Newton at the end of his "Optics" contain more grand and speculative views that might be brought to bear upon this question than any found in the works of modern electricians†; but it is very unjust to the experimentalists who, by the laborious application of new instruments, have discovered novel facts and analogies, to refer them to any such suppositions as, "that all attractions, chemical‡, electrical, magnetic, and gravitative, may depend upon the same cause;" or to still looser

* As a specimen of the *Prolusiones*, I shall give a few articles from the index, which will show the character of the work. *Prolusiones*, pag. 256, et seq.

256. "Adamas est Andronia.

260. "Andronia cum Plumbo creat Barytam, cum Ferro Chalybem.

262. "Carbo est acidus cum Atmosphæra basica.

263. "Chromium non est nisi Calx Magnesii acida.

——. "Cuprum cum Andronià coalescit in Moybdænum.

268. "Scintilla electrica formatur à Principiis Conductorem primum et secundum animantibus, ac inter se concurrentibus; est gravis, habet effectum electricitati contrarium."

† See the eloquent observations of M. Chenevix on the subject of Winterl's Theory, *Annales de Chimie*, vol. 50, 2 cap. 175.

‡ In the *Système Universelle* of M. Azais, not only are all the phænomena of nature referred to the same cause, but specific reasonings upon the mode of its operation given. In this work, published in 1810, not only is the identity of magnetism and electricity insisted on, but an attempt is made to explain the manner in which the two electrical fluids produce the magnetic phænomena, pag. 239, vol. i. "Ainsi ces deux ordres de phénomènes sont tres resemblans. Repetons que toutes leurs différences résultent uniquement de ce que les deux fluides sont moins intenses lorsqu'ils produisent les phénomènes du Magnétisme que lorsqu'ils produisent les phénomènes du Galvanisme, &c." It requires only the same principle as that censured in the text to refer to this author the discovery of Oersted and the speculations of Ampère. M. Azais, in his "fluides mineure et majeure," finds all the causes of the acid and alkaline properties of bodies:—slow combinations, the heat produced, and all the phænomena of chemical change; and his reasonings are often very ingenious.

 expres-

expressions, in which different words are used and applied to the same ideas, and in which all the phænomena of nature are supposed to depend on the Dynamic system, or the equilibrium and opposition of antagonist powers.

The true origin of all that has been done in electro-chemical science was the accidental discovery of MM. Nicholson and Carlisle, of the decomposition of water by the pile of Volta, April 30, 1800*. These gentlemen immediately added to this capital fact, the knowledge of the decomposition of certain metallic solutions, and the circumstance of the separation of alkali on the negative plates of the apparatus. Mr. Cruickshank, in pursuing their experiments, added to them many important new results, such as the decompositions of muriates of magnesia, soda and ammonia, by the pile; and that alkaline matter always appeared at the negative, and acid at the positive pole †: and Dr. Henry about the same time made some unsuccessful attempts to decompose potassa in solution by the pile, and confirmed the general conclusions of MM. Nicholson, Carlisle, and Cruickshank. In the month of September in this year, I published my first paper on the subject of Galvanic Electricity, in Nicholson's Journal, which was followed by six others ‡: the last of which appeared in January 1801. In these papers I showed that oxygen and hydrogen were evolved from separate portions of water, though vegetable and even animal substances intervened; and conceiving that all decompositions might be polar §, I electrised different compounds at the different extremities, and found that sulphur and metallic substances appeared at the negative pole, and oxygen and azote at the positive pole, though the bodies furnishing them were separate from each other. In the same series of papers I established the intimate connexion between the electrical effects and the chemical changes going on in the pile, and drew the conclusion of the dependence of one upon the other. In 1802 I proved that galvanic combinations might be formed from single metals, or charcoal and different fluids chiefly acid and alkaline, and that the side or pole of the conducting substance in contact with the alkali was positive, and that in contact with the acid, negative; and in the same year I published, that when two separate portions of water, connected by moist bladder or muscular fibre, were electrised, nitro-muriatic acid appeared at the positive, and fixed alkali at the negative pole §. In the same year Dr. Wollaston placed the identity of the cause of

* Nicholson's Journal, vol. xlii. p. 183. † Ibid. vol. iv. p. 190.

‡ Ibid. pp. 275, 326, 337, 394, 380.

§ Journal of the Royal Institution, 1802. First Series.

galvanism

galvanism and electricity, which had been always maintained by Volta, out of all doubt, by some very decisive experiments.

In 1804, MM. Hisinger and Berzelius stated that neutro-saline solutions were decomposed by electricity, and the acid matter separated at the positive, and the alkaline matter at the negative poles; and they asserted that in this way muriate of lime might be decomposed; and drew the conclusion that nascent hydrogen was not, as had been generally believed, the cause of the appearance of metals from metallic solutions. These valuable observations ought to have explained distinctly the source of the appearance of acid and alkaline substances at the two extremities of the pile; yet the paper was never translated into English, nor at all attended to; and one of their facts was contradicted by the assertion of, generally, a very accurate observer, Mr. Cruickshank, who in his early experiments mentioned that he had not been able to decompose muriate of lime in the circuit.

In 1805 various statements were made, both in Italy and England, respecting the generation of muriatic acid and fixed alkali from pure water. The fact was asserted by MM. Pacchioni and Peele, and denied by Dr. Wollaston, M. Biot, and the Galvanic Society at Paris*. Mr. Sylvester, who conducted his experiments with some care, stated, that if two separate portions of water were electrised out of the contact of substances containing alkaline or acid matter, acid and alkali were generated; so that up to this time the question, whether these substances were liberated from their combinations, or formed from their elements by electricity, could not be considered as decided: a circumstance not so much to be wondered at, when the novel and extraordinary nature of the whole class of galvanic phænomena is considered.

It was in the beginning of 1806 † that I attempted the solution of this question; and after some months' labour I presented to the Society the Dissertation, to which I have re-

* Some writers have very incorrectly referred the origin of these researches to the observations of Hisinger and Berzelius; *Annales de Chim.* vol. li. 1 cap. pag. 167; but these observations were never quoted by any writer of the day on the pretended production of muriatic acid and alkali; and I was not acquainted with them till after my fundamental experiments were finished; and, when in drawing up an account of them, I looked back through the whole series of periodical publications to find accounts of experiments bearing upon the same question, and I believe I first directed the public attention to the value of those researches. Whoever will take the trouble to read the Bakerian Lecture for 1806, will be convinced of the gradual development of the whole subject from the investigation respecting the pretended formation of muriatic acid and fixed alkali.

† Phil. Trans. 1807.

ferred in the beginning of this Lecture. Finding that acid and alkaline substances, even when existing in the most solid combinations, or in the smallest proportion in the hardest bodies, were elicited by voltaic electricity, I established that they were the results of decomposition, and not of composition or generation; and referring to my experiments of 1800 and 1801 and 1802, and to a number of new facts, which showed that inflammable substances and oxygen, alkalies and acids, and oxidable and noble metals, were in electrical relations of positive and negative, I drew the conclusion, *that the combinations and decompositions by electricity were referrible to the law of electrical attractions and repulsions,* and advanced the hypothesis, "*that chemical and electrical attraction were produced by the same cause, acting in one case on particles, in the other on masses;" and that the same property, under different modifications, was the cause of all the phænomena exhibited by different voltaic combinations.*

Believing that our philosophical systems are exceedingly imperfect, I never attached much importance to this hypothesis; but having formed it after a copious induction of facts, and having gained immediately by the application of it a number of practical results, and considering myself as much the author of it as I was of the decomposition of the alkalies, and having developed it in an elementary work, as far as the present state of chemistry seemed to allow, I have never criticized or examined the manner in which different authors have adopted or explained it,—contented, if in the hands of others it assisted the arrangements of chemistry or mineralogy, or became an instrument of discovery. And having now given what I believe to be a faithful sketch of its origin, I shall not enter into an examination of those works which have induced me to make this sketch, and which contain partial or loose statements on the subject, and which refer the origin of electro-chemistry to Germany, Sweden and France, rather than to Italy and England, and which attribute some of the views of the science, which I first developed, to philosophers who have never made any claim of the kind, and who never could have made any, as their works on the subject were published many years after 1806.

III. *On the modes adopted for detecting the electrical states of bodies, and definitions of terms.*

That the statements made in the following sections may be more distinct, I shall say a few words of the mode in which the different conditions of electrical action were ascertained, and

and describe the manner in which I have used the terms which have been adopted in electro-chemical science.

In determining the nature of the electrical action in what may be called the closed circle, or the combinations in which, according to the language used on the continent, electrical currents exist, I have employed instruments constructed upon the same principles as the galvanometer of Professor Cumming, or the multiplier of Professor Schweigger. Silver wire, covered with silk, about 1-70th of an inch in diameter, was folded round a small wooden frame, so as to fill a narrow deep groove: the two extreme wires were parallel, and the convolutions as nearly as possible in the same perpendicular: a small tube containing a filament of silk was passed through the centre of the convolutions of wires, to which a delicate magnetised needle was suspended; which, when the apparatus was properly disposed, rested with its north pole between the two extremities of the wires. This instrument, which contained 60 circumvolutions of wire, was found sufficiently delicate for most purposes of experiment; but in a few instances, in which very weak electricities were to be determined, I used another apparatus, in which the same kind of wire was fastened, in concentric circles, round two portions of glass tube, in such a manner that radii from the inner circle would have passed through all the wires, and in which increased mobility was given to the system by two needles exterior to it and connected with it, placed one above, the other below the central needle, with their poles in the same directions, but opposite to those of the central needle, which was so magnetised that its directive power was neutralised by the power of the other two needles *.

To illustrate the operation of these apparatus, I shall state, that when the lower terminating wire, which was to the left, or east of the north pole, was connected with a piece of zinc, and the upper one with a piece of platinum, both being in common water, the deviation of the central needle was eight or ten degrees, the south pole turning to the east or left hand; which may be considered as indicating that the current of electricity was from the platinum to the zinc through the wire, and that the surface of the zinc in the fluid was positive with respect to the opposite surface of platinum; and in using the terms positive and negative, I beg to be understood as applying them to the metallic surfaces in contact with the fluid.

* This arrangement differs from that of M. Nobile only by a duplication of effect.

For

For determining weak electricities of charge, or as it is sometimes called, of tension, I used Volta's condenser connected with Bennet's electrometer, and sometimes with one constructed on the principle of Behrens, consisting of an insulated gold leaf, or what I found better, a silk filament, made conducting by impalpable charcoal powder, to receive the charge, placed between the poles of a dry pile consisting of 400 circles of silver and gold foil, of the third of an inch in diameter, or 50 of zinc and silver of the same size, with paper intervening; the attraction of the gold leaf or the filament, either to the positive or negative pole, indicates the nature of the charge: and, as in cases of electro-chemical action there are always two corresponding opposite states, I considered the part of the system which touched the conductor as possessing the same electrical state with that exhibited by the leaf. I have never however put much dependence upon indications given by this instrument, unless they were confirmed by other results; having found them very uncertain, and influenced by the state of the condenser and the atmosphere.

[To be continued.]

XI. *Observations on a Mineral from near Hay Tor, in Devonshire.* By Cornelius Tripe, *Esq.**

WHILE mineralogical science is so extensively cultivated, and rapidly increasing in interest and importance, the following brief description of a newly discovered mineral substance may not be unacceptable to the readers of the Philosophical Magazine.

The mineral alluded to was found in detached pieces, accompanied by small masses of chalcedony, garnet, actynolite, talc, and very splendent octohedral oxidulated iron. These substances, altogether, formed a single bunch of inconsiderable size enveloped by a ferruginous clay, in a large lode of very pure oxidulated iron, in an iron mine adjacent to the Hay Tor granite quarries, Devonshire.

Mr. Kennard, a respectable mineral dealer of Devonport, personally assisted in removing the mineral from its situation, and possesses the greater part which has been raised. I have myself examined the mine since; but with the most attentive observation could not detect any further trace of the substance, nor of the massive chalcedony, actynolite, or garnet, which, as already stated, was found associated with it.

The crystals, which are generally large and well defined,

* Communicated by the Author.

are

are of a brownish red, ferruginous yellow, and delicate white colour. Every crystal has *certain* planes smooth and splendent, while the *others* are rough and dull, and is either semi-transparent or translucent. The substance scratches rock crystal, and in lustre, colour, fracture, and general appearance closely resembles chalcedony.

Mr. Robert Cole, a gentleman resident here, who is passionately devoted to the science, has been associated with us in examining the mineral; and after giving it our best consideration, we have been led to conclude that it is crystallized chalcedony. Its hardness, fracture, colour, and close affinity in appearance to that substance, warrant such opinion; whilst the uniformity of character observable throughout the whole series of specimens, with the perfection and brilliancy of many of the crystals, induce us to believe that the crystallization is *original*. We are aware that chalcedony has not hitherto been found *regularly crystallized;* but this we submit does not present an insuperable barrier to the opinion just expressed, unless a boundary or limit be set up to the operations of nature. Chemical analysis, however, might detect some variation from the common constituents of chalcedony, and which might entitle this mineral to be ranged in the catalogue of *new substances*. And with these considerations we contemplated calling it *Haytorite*, in honour of its birth-place.

Some specimens have been presented to Mr. Wm. Phillips, who, with a candour characteristic of true philosophy, immediately furnished us with the drawings and measurements forming the subject of the ensuing communication, together with some observations of his own. We think it right, however, to state that we have been favoured with an opinion, that the crystals are pseudomorphous crystals of chalcedony, since no cleavage can be obtained; and that, as their figure very nearly approximates that of sphene, it has been conjectured they have derived their form from that substance. In reply to this opinion it may be stated, that no trace of sphene has hitherto been discovered in the mine; neither have the individual substances of which sphene is compounded, except silex,—unless we take into account the small quantity of lime which is in the garnet. And although no regular cleavage has been obtained,—the *brilliancy*, *uniformity* and *perfection* of the crystals, might incline to the opinion that they are possessed of *regular structure*, but so constituted as to render it more then ordinarily difficult to discover.

We submit these observations with much deference: but as they appear to us to stand in the way of the opinion just alluded to, and to convey a doubt whether the interesting substance

stance which we have been considering should be ranked as a *pseudomorphous* production, a *regularly* crystallized chalcedony, or a *new* substance,—we therefore deemed it right to state them.

Devonport, Nov. 25, 1826.

XII. *Remarks on the Crystalline Form of the Haytorite.* By W. Phillips, *F.G.S. &c.**

I HAVE received from the writer of the foregoing communication, and from the other gentlemen who are therein mentioned, several crystals of the substance to which they have given the appropriate name of *Haytorite.*

It has only been found in regular crystals, which in general are well defined, the edges being sharp, and the planes for the most part brilliant. In dimension they vary from the size of a pin's head to an inch in diameter: three or four minute crystals are colourless and almost perfectly transparent; but in general their colour passes from pale brownish yellow, (in which case they are translucent,) to deep brown and opaque.

The crystals, however, have rarely been found isolated, being commonly grouped together in such a manner as to show only about one half of the crystal, but they are easily separable; the planes of separation are bright and frequently somewhat iridescent on the surface.

I have in vain attempted to discover a regular cleavage, which rarely is absent in crystallized minerals; and it is remarkable that the surface produced by breaking a crystal in any direction, is almost totally devoid of lustre, having completely the aspect and fracture of chalcedony: and this takes place even in the almost perfectly transparent crystals, which lose immediately that character, assuming the same degree of translucency as is commonly possessed by chalcedony, when viewed on the fractured surface. Specific gravity of two translucent crystals taken by my friend S. L. Kent, F.G.S. 2·5628 . 2·5862. It scratches quartz.

The characters detailed in the preceding sentence induced the suspicion,—and which I communicated to Messrs. Tripe and Cole in my first letter to them on the subject,—that it is only a pseudomorphous mineral.

Whether such be its real character, or whether it is to be considered a new mineral, its primary form (assuming the planes P and k k^1 as primary) is an oblique rhombic prism, differing less than one degree from the proportions of a right rhombic prism, and of which the lateral planes meet at the

* Communicated by the Author.

angles

angles of 77° and 103°: the terminal plane declining from one acute angle to the other. Some of the crystals forming one large group in my possession, are opaque, in others translucency exists; and others again seem to have suffered a partial decomposition, having the appearance of being carious internally; but to what extent soever that appearance has taken place, it is remarkable that the portion remaining of the external plane, however small, is not deprived of its ordinary lustre, and often is even brilliant.

Now if this apparent injury had been the real effect of decomposition, we might expect that the agent producing it would in the first place have acted externally, and thus have deprived the external planes of their natural brilliancy; and this consideration again tempted the further examination as to whether there existed in the crystals, thus partially hollow, any stalactitical appearance of chalcedonic matter: this certainly does appear on a close examination by the help of a glass; a circumstance amounting almost to proof that the crystals are in reality pseudomorphous, and that their substance is chalcedony: the smaller crystals were sometimes enveloped by it.

But if pseudomorphous, the next question that arose was, What was the original substance of which the chalcedony had taken the form: and being on this point completely at fault, I showed a crystal to my friend H. J. Brooke, Esq. which he submitted to the examination of Mr. Lévy, who suggested that the original substance might have been sphene; and hence the suspicion already alluded to by Mr. Tripe.

The primary form of sphene is likewise an oblique rhombic prism; but that which has hitherto been so considered, differs greatly in measurement from the apparent primary form of Haytorite. M. Lévy will, perhaps, add his opinions on the subject.

Of the following figures, fig. 1. represents the ordinary form of the large crystals, the planes *r* and *c* being always striated, *s* always convex, *d* mostly rough and dull, *k* always smooth, but dull. Fig. 2. represents a smallish crystal, with bright planes except *k*, which is striated as in the figure. Figs. 3. and 4. represent two transparent crystals scarcely larger than flattened pins' heads, of which the planes are all bright, except *k*. In the present uncertain origin of this mineral, I have thought it better to give arbitrary signs to the several planes, than to adopt the notation of Mr. Brooke.

Haytorite.—*Measurements by the reflective Goniometer.*

P on	*d*	135° 5′		g^1 on	g^2	160° 38′
—	*c*	134 55		g^1 on	*h*	157 30
—	*g*	147 38		—	*l*	156 50
—	g^2	128 22		—	*v*	139 42
—	*h*	141 20		g^2 on	*l*	150 8
—	*i*	90 3		*k* on	k^1	77 00
—	*k*	90 20		*k* on	*m*	128 30
—	*l*	141 25		*l* on	*v*	162 25
—	*m*	90 14		*v* on	v^1	130 22
—	*n*	116 42		*m* on	*o*	157 20
—	*v*	130 5		—	*i*	147 40
d on	*h*	140 32				

Fig. 1.

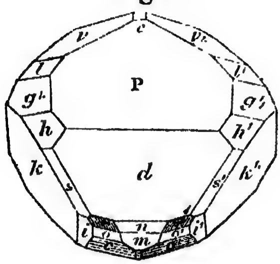


Fig. 2.

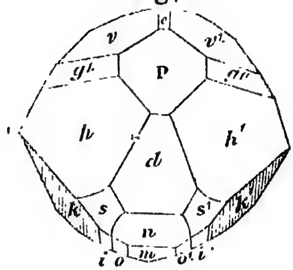


Fig. 3.

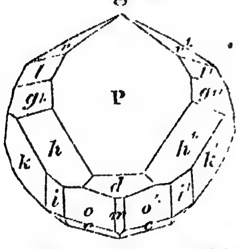
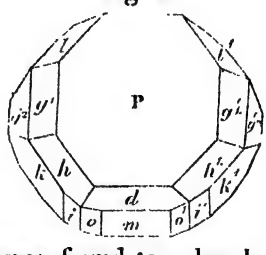


Fig. 4.



The vein in which this substance was found, as already mentioned by Mr. Tripe, is of oxidulated iron; and having lately had an opportunity of seeing that vein, I may add that it is about 6 feet wide, runs nearly east and west, and underlies to the north about 45°, or 6 feet in a fathom. The iron is occasionally free from admixture, and crystallized in octohedrons; but, as was perceptible in the cavity made in the vein from the surface by the miner, and which is about 30 feet long by 12 to 15 in depth, the mass of ironstone is commonly mixed with what appears by the help of the glass to be a species of actynolite, of a greenish colour, and thus affording that

that tint to the mass. It is shipped at Teignmouth, but for what place I could not learn. This vein appeared to me the more worthy of notice, as being the only one I ever met with of that substance. It is only about two miles from the vast and extraordinary deposition of wood coal near Bovey, which possibly might be advantageously employed in smelting it. The vein of ironstone lies in slates, the killas of the Cornish miner, but immediately reposes on a stratum of a substance termed provincially Irestone (ironstone) from its excessive hardness. It appears to be siliceous schist, and abounds so greatly in some mines east of Truro in Cornwall, as to prove greatly injurious to the miner.

XIII. *On the Origin of the Crystalline Forms of the Haytorite. By* A. Lévy, *Esq. M.A. F.G.S.**

MR. W. PHILLIPS having been so good as to communicate to me the drawings and measurements of the crystals of Haytorite he has given in the preceding paper, I first examined whether by taking a sufficient number of the observed angles as data to determine the dimensions of the simplest form which may be assumed as the primitive, the other observed incidences did agree with the results of calculation of simple decrements. Because if that was not the case (as no doubt can be entertained on the accuracy of the observations) it would follow, in the supposition of these crystals being pseudomorphic, that they had not preserved exactly the angles of the substance upon which they had been modelled. Consequently no perfect agreement could be expected between the incidences they afford, and those of any mineral; and at the same time that it would make it difficult to decide which was the substance they had replaced, a similarity of their forms, and not too wide a difference of their angles, with those of a crystallized species, would be sufficient to make it probable that they were pseudomorphic crystals of it. Inferences entirely opposite to the preceding would be drawn, if the equality between the observed and calculated angles should obtain.

Now the forms, and mere inspection of the *ensemble* of the measurements of Haytorite prove, that we may assume for the primitive form an oblique rhombic prism, the lateral planes of which would correspond to the planes g^1, and the base to the plane m (See the figures in the preceding paper); this base being inclined upon the lateral planes at an angle very

* Communicated by the Author.

 little

little greater than a right angle *. I have determined the dimensions of this primitive form by means of the observed angles P on g^1, P on d, and P on e. Supposing besides that the faces d and e are both the result of a decrement by two rows in breadth, the one on the angle o and the other on the angle a of the primitive; I find then that the planes Mr. Phillips has designated by m, g^1, g^2, P, e, n, d, k, i, o, l, v, h, which are the only ones for which he has given measurements, may be represented respectively by the following simple crystallographical signs, P, m, g^3, h^1, a^2, o^2, o^4, e^1, e^2, e^3, b^1, $b^{\frac{1}{2}}$, $d^{\frac{1}{2}}$. Their calculated incidences are written below, and by the side of them their differences with the observations.

	Diff. with Observations.		Diff. with Observations.
$m, m = 115° 16'$			
$P, m = 90° 8' 30''$		$P, e^1 = 128° 39' 30''$	. . $9' 30''$
$m, h^1 = 147° 38'$	. . . 0	$P, e^2 = 147° 30'$	. $-10'$
$P, h^1 = 90° 10'$	. . $-4'$	$P, e^3 = 157° 22' 40''$	. . $2' 40''$
$m, g^3 = 160° 38'$	. . . 0	$m, b^1 = 139° 45'$	. . . $3'$
$h^1, o^1 = 135° 5'$	. . . 0	$, b^{\frac{1}{2}} = 157° 5'$	. . . $15'$
$h^1, a^2 = 134° 55'$	. . . 0	$m, d^{\frac{1}{2}} = 157° 7' 30''$	. $-22' 30''$
$h^1, o^4 = 116° 42'$	. . . 0	$h^1, d^{\frac{1}{2}} = 141° 7' 30''$	$-12' 30''$

The above differences are so small that it may be inferred at once that the crystals of Haytorite are perfectly regular, and that one of the forms which may be taken as their primitive can differ but very little from that above assumed; that is to say, from an oblique rhombic prism the incidence of the lateral planes of which is 115° 16', the incidence of the base on each of them 90° 8' 30'', and the lateral edge equal to the oblique diagonal of the base. It is also obvious that if the primitive was supposed to be a right rhombic prism, preserving still the same incidence for the lateral planes, the differences between the observations and the results of calculation would not be much greater than above. But I think, however, that it is better to consider the primitive as a slightly oblique rhombic prism, because all the measurements of Mr. Phillips indicate an inclination of the base in the same direction, and also because the symmetry of the crystals agrees better with it.

When I first saw the drawings and measurements of Haytorite, I thought they might be considered as pseudomorphic crystals of sphene, because some of the angles are not very far from those of that substance. But the preceding investigation proves, that an almost perfect equality must be established between the angles of Haytorite and those of any other mineral, before it can be reasonably suspected that the

* The drawings to be put in position with respect to this primitive form should be inverted.

crystals

crystals of the first have only borrowed the forms of the other; and therefore the above suggestion must be abandoned. The only substance between the angles of which and those of Haytorite there seems to be a great analogy is Humboldtite. First, by inverting the drawings of Mr. Phillips, the similitude of the forms of Haytorite with that of Humboldtite represented by Mr. Phillips, (see the Elementary Introduction to Mineralogy, p. 380,) or the one I have given in the number of the Annals of Philosophy for February 1823, becomes apparent. The planes P, g^1, d, h, k, i, n, v of Haytorite corresponding to the planes of Humboldtite, Mr. Phillips has designated in the work above referred to, by $h, m, a_3, f, e_2, e_1, a^1, g^1$: the incidences of these different planes measured by Mr. Phillips are,

Haytorite.	Humboldtite.
P, g^1 = 147° 38′	m, h = 147° 50′
P, d = 135° 5′	h, a^3 = 133° 56′
g^1, h = 157° 30′	m, f = 156° 50′
k, k = 77°	e_2, e_2 = 77° 25′
k, i = 160° 50′	e_1, e_2 = 161° 20′
P, n = 116° 42′	h, a^1 = 116° 20′
g^1, v = 139° 42′	m, g^1 = 138° 5′
P, h = 141° 20′	h, f = 140° 50′

The agreement between these several angles is not perfect: but it must be observed that some doubt may still be entertained with respect to the angles of Humboldtite. Some of the measurements of that substance given by Mr. Phillips, indicate that the base of the primitive makes an angle less than 90° with the plane h; whilst from the observations I took as data, I found that angle equal to 91° 41′ 30″ *. There is not therefore

* Professor Mohs and Mr. Haidinger consider Humboldtite as a variety of Datholite; and their opinion is supported by a great analogy between the two substances. However, I think there can be no inconvenience to preserve the distinction till some better measurements of Humboldtite can be obtained, especially as it appears to me there are still some reasons to make doubtful the propriety of the re-union. In order to refer the crystals of Datholite and Humboldtite to the same primitive form, the incidences of the planes of Humboldtite I have designated by e^1, and Mr. Phillips by e^2, should be the same as the incidence of the planes M of Datholite. The first incidence I state in the paper inserted in the Annals of Philosophy for February 1823, was one I could obtain most accurately, and I found it equal to 102° 30′: the second I found 103° 25′. Mr. Phillips in his elementary work on Mineralogy gives the result of his own observations of these two angles 102° 35′, and 103° 40′. At my request he was so good as to take again some measurements of Datholite, and found M on M 103° 40′, 103° 38′, 103° 35′. The agreement in the amount of the difference of these two angles, by two different observers, is perhaps sufficient to throw some doubt upon the accuracy of the opinion founded on their equality. To a certain extent

therefore, sufficient evidence perhaps to say that the crystals of Haytorite are pseudomorphic of Humboldtite. The repositories of the two substances seem to be different: for besides the already known localities of Humboldtite,—the Seiser alp in the Tyrol, and Salisbury-craig near Edinburgh,—I know only of another, which is Utoë in Sweden, where, to judge by the specimen in Mr. Heuland's collection, it occurs in macled crystals, and accompanied by apophyllite, carbonate of lime, sulphate of barytes, and bitumen.

In conclusion it may be said, that if the reasons for supposing the crystals of Haytorite to be pseudomorphic appear conclusive, there is some not unreasonable ground to think they may owe their form to Humboldtite, but have been modelled upon crystals of that substance larger and of a different variety than those which have been met with hitherto; or otherwise they must be considered as pseudomorphic crystals of an unknown species.

XIV. *Astronomical Observations*, 1826. *By Lieut.* Beaufoy, *R. N.*

Bushey Heath, near Stanmore.

LATITUDE 51° 37′ 44″ 3 North. Longitude West in time 1′ 20″ 93.

Nov. 29, Solar Eclipse. Beginning 21^h 46′ 04″ M. T. Bushey.
End . . . 23^h 58′ 19″ M. T. Bushey.

No spots were visible on the sun's disc. The edge of the moon uneven, and her horns blunted.

Dec. 3. An occultation of a small star by the moon . . . } 22^h 44′ 40″ 6 Sid. Time.

extent it may be said, that the smaller the difference between the measurements of two crystals, if well established, the greater is the difficulty to refer them to the same primitive form. If therefore the difference which Mr. Phillips and myself have found should prove to be correct, it would be most likely an insurmountable objection to the re-union of Humboldtite with Datholite. There is not a much greater difference between the crystals of Cleavelandite and Felspar, than there would be between those of these two substances. There is still another way to compare the crystals of Humboldtite with those of Datholite: it is by supposing that the planes M of the first correspond to the planes *i* of the second (see Phillips's figures); for according to Mr. Phillips they measure exactly the same angles; but then a difficulty of the same order as the one above would be raised, with respect to the identity of the faces e^2 of Humboldtite and a^2 of Datholite.

XV. *A List*

XV. *A List of Moon-culminating Stars for* 1827*.

Dear Sir, London, 25 Aug. 1826.

I SEND you inclosed the list of moon-culminating stars for the first six months of the ensuing year 1827. The method I adopt is to look out for two stars to *precede*, and two to *follow* the moon's transit: one of *each* of these is to be as near as possible to the moon in right ascension; and the others at not more than 30 or 40 minutes distance.—The former are for the use of those observatories that are situated near each other; such as the principal European ones; and the others are for observatories situated at a considerable distance from each other, and for travellers in various parts of the world.—I have also selected stars of *greater* magnitude than have generally been adopted: for I have found by experience that the smaller stars are apt to be mistaken; and several erroneous observations have consequently been recorded. For that purpose I have taken a greater range in declination (still however limiting that range to a difference of 2° or 3° only); for I find that unless a transit instrument is very much *out of level*, no great error can arise. In these latitudes an inclination of the axis, of 10″, would make a difference of only 0″,01 in time, when the star is situated 1° from the moon: and as we approach towards the equator, these differences vanish altogether.

You will observe that I have inserted Jupiter and Saturn, when they are near the moon, and when their motion is *retrograde:* and also Venus on the day of her occultation in February. I have also inserted the moon's declination as well as her right ascension: both of which relate to the time of her culmination at Greenwich. Opposite to the moon I have also placed (in a parenthesis) the number of days, from the time of new moon; which may be occasionally useful.

Those stars to which an asterisk is annexed, are such as will pass through the field of the telescope (in these latitudes) on the same apparent parallel as the moon: without any alteration in the position of the telescope: and such stars will probably undergo an occultation in the course of the evening. They are pointed out as more worthy of observation for fixed observatories in this part of Europe. I am, &c.

FRANCIS BAILY.

[* From Schumacher's *Astronomische Nachrichten*, No. 106. Most of the stars, after the 17th day of the Moon's age, have been added by M. Clausen: as Mr. Baily considered it useless to select any stars for those days unless they were of the 1st or 2nd magnitude: and had not selected any stars differing more than 3° from the Moon, in declination. The numbers within parentheses are from Piazzi's Catalogue.—EDIT.]

1827.	Stars.	Mag.	Æ. h	m	s	D. °	′
Jan. 3	18 λ Piscium ..	5	23	33	13	+ 0	50
	19 Piscium	6		37	33	2	32
	Moon	(6)		42		2	49
	22 Piscium......	6		43	6	1	58*
	28 ω Piscium....	4·5		50	26	5	54
4	51 Piscium.......	6·7	0	23	29	+ 6	0
	Moon	(7)		31		7	14
	63 δ Piscium	5		39	43	6	39*
	71 ε Piscium	4		53	59	6	57
5	(8) Piscium.......	7	1	2	26	+ 9	22
	Moon	(8)		20		11	14
	102 π Piscium ..	6		27	57	11	15
6	19 Arietis	7	2	3	38	+14	28
	Moon..........	(9)		9		14	41
	27 ψ Arietis	6		21	19	16	56
7	42 π Arietis	5	2	39	38	+16	44
	46 ρ³ Arietis	6		46	41	17	20
	Moon	(10)		58		17	26
	57 δ Arietis	4	3	1	45	19	4
8	13 F¹ Tauri	6·7	3	32	11	+19	8*
	Moon..........	(11)		49		19	25
	37 A¹ Tauri	5		54	29	21	36
	43 ω¹ Tauri	6		59	6	19	9*
	50 ω² Tauri	5·6	4	7	8	20	9
9	87 α Tauri	1	4	26	2	+16	9
	94 τ Tauri......	5		31	52	22	37
	Moon	(12)		41		20	30
	102 ι Tauri	4·5		52	46	21	20
10	114 ο Tauri	5	5	17	15	+21	47
	123 ζ Tauri	3·4		27	18	21	2
	Moon	(13)		34		20	37
	54 χ¹ Orionis ..	5		44	8	20	14*
11	*Saturn*		6	6		+22	33
	13 μ Gemin.....	3		12	29	22	36
	18 ν Gemin.	5		18	41	20	19
	Moon	(14)		27		19	44
	26 u Gemin.	5·6		32	20	17	48
12	54 λ Gemin.	4·5	7	8	9	+16	51
	Moon	(15)		19		17	52
	68 k Gemin.	5		23	44	16	12
	1 Cancri	6		47	9	16	15
13	12 s Cancri	6	7	59	2	+14	8
	Moon..........	(16)	8	13		15	6
	27 e Cancri	6·7		17	9	13	13
	65 α² Cancri	5		49	1	12	31
14	65 α² Cancri	5	8	49	1	+12	31
	76 κ Cancri	5·6		58	23	11	21

1827.	Stars.	Mag.	Æ. h	m	s	D. °	′
Jan. 14	*Moon*	(17)	9	5		+11	32
	5 ξ Leonis	5		22	37	12	3
	•14 ο Leonis	4		31	54	10	40
19	7 δ Corvi	3·4	12	20	56	−15	33
	67 α Virginis	1	13	16	5	10	15
	Moon	(22)		23		11	14
20	67 α Virginis	1	13	16	5	−10	15
	Moon	(23)	14	20		11	14
	9 α^2 Libræ	3		41	19	15	19
21	9 α^2 Libræ	3	14	41	19	−15	19
	20 γ Scorpii	3·4		53	58	24	36
	Moon	(24)	15	20		18	13
22	7 δ Scorpii	3	15	50	7	−22	7
	8 β^1 Scorpii	2		55	23	19	19
	Moon	(25)	16	23		20	5
Feb. 2	102 π Piscium	6	1	27	57	+11	15
	Moon	(7)		49		13	6
	19 Arietis	7	2	3	38	14	28
3	27 ψ Arietis	6	2	21	19	+16	56
	Moon	(8)		39		16	11
	(203) Arietis	7		43	34	15	46*
	53 Arietis	6		57	42	17	12
	57 δ Arietis	4	3	1	45	19	4
4	57 δ Arietis	4	3	1	45	+19	4
	(60) Tauri	7		17	11	18	8*
	Moon	(9)		29		18	30
	(215) Tauri	6·7		50	51	17	42
5	43 ω^1 Tauri	6	3	59	6	+19	9
	74 ε Tauri	4	4	18	31	18	47
	Moon	(10)		20		19	58
	87 α Tauri	1		26	2	16	9
	94 τ Tauri	5		31	52	22	37
	102 ι Tauri	4·5		52	46	21	20
6	102 ι Tauri	4·5	4	52	46	+21	20
	109 n Tauri	5·6	5	8	53	21	55
	Moon	(11)		13		20	29
	114 ο Tauri	5		17	15	21	47
	123 ζ Tauri	3·4		27	18	21	2
7	54 χ^1 Orionis	5	5	44	8	+20	14
	Saturn	....		58		22	38
	Moon	(12)	6	5		20	2
	18 ν Gemin.	5		18	41	20	19
	24 γ Gemin.	3		27	43	16	32
8	24 γ Gemin.	3	6	27	43	+16	32
	43 ζ Gemin.	4		53	51	20	49
	Moon	(13)		58		18	36
	54 λ Gemin.	4·5	7	8	9	16	51

1827.	Stars.	Mag.	Æ.			D.	
Feb. 9	74 f Gemin. ..	6	7	29	29	+18	4
	1 Cancri	6		47	9	16	15
	Moon	(14)		51		16	14
	25 d² Cancri	6	8	16	1	17	36
10	29 Cancri	6	8	18	57	+14	46
	50 A² Cancri...	6		37	26	12	44
	Moon	(15)		43		12	59
	65 α^2 Cancri...	5		49	1	12	31*
	76 κ Cancri ...	5·6		58	23	11	21
11	5 ξ Leonis.....	5	9	22	37	+12	3
	14 o Leonis ...	4		31	54	10	40
	Moon.........	(16)		35		9	2
	29 π Leonis ...	4·5		51	4	8	52
	32 α Leonis ...	1		59	12	12	48
12	16 Sextantis ...	6	10	0	10	+ 7	0
	32 x Sextantis .	7		23	19		31
	Moon.........	(17)		29		4	33
	36 n Sextantis .	6		36	14	3	13
	55 Leonis.....	6		46	48	1	39
13	62 g Leonis ...	6	10	54	45	+ 0	55
	69 Leonis.....	5·6	11	4	54	+ 0	52
	Moon.........	(18)		21		− 0	14
	91 υ Leonis ...	4·5		28	5	+ 0	8
15	*Moon*	(20)	13	7		− 9	42
	67 α Virginis .	1		16	8	10	15
18	7 δ Scorpii ...	3	15	50	7	−22	7
	8 β^1 Scorpii ...	2		55	23	19	19
	Moon.........	(23)	16	4		19	23
19	21 α Scorpii ...	1	16	18	49	−26	2
	23 τ Scorpii ...	3·4		25	7	27	51
	Moon.........	(24)	17	6		20	20
20	*Moon*.........	(25)	18	9		−19	53
	22 λ Sagitt. ...	3·4	18	17	18	25	30
	34 σ Sagitt. ...	3		44	32	26	30
21	22 λ Sagitt. ...	3·4	18	17	18	−25	30
	34 σ Sagitt. ...	3		44	32	26	30
	Venus		19	8		18	39*
	Moon.........	(26)	19	11		18	8
Mar. 5	94 τ Tauri ...	5	4	31	52	+22	37
	97 i Tauri.....	5·6		41	15	18	32
	Moon.........	(8)		51		20	9
	107 l² Tauri ...	7		58	38	19	37*
	109 n Tauri ...	5·6	5	8	53	21	55
	119 Tauri	5·6		22	4	18	27
	123 ζ Tauri ...	3·4	5	27	18	+21	2
	57 χ^2 Orionis .	6		44	43	19	43*
	Moon.........	(9)		44		20	6

1827.	Stars.	Mag.	Æ. h	m	s	D. °	′
Mar. 6	64 χ^4 Orionis ..	5·6	5	53	13	+19	41*
	18 ν Gemin.	5	6	18	41	20	19
7	18 ν Gemin.	5	6	18	41	+20	19
	26 u Gemin. .. .	5·6		32	20	17	48
	Moon	(10)		36		19	7
	(281) Gemin.....	7		47	38	18	7
	43 ζ Gemin.	4		53	51	20	49
8	54 λ Gemin.....	4·5	7	8	9	+16	51
	68 k Gemin.	5		23	44	16	11
	Moon	(11)		28		17	11
	81 g Gemin.....	6		36	6	18	55
	5 r Cancri	6		51	38	16	55
9	16· ζ Cancri	6	8	2	17	+18	9
	29 Cancri	6		18	57	14	46
	Moon	(12)		20		14	21
	45 A^1 Cancri....	6·7		33	39	13	17
	65 α^2 Cancri	5		49	1	12	31
10	65 α^2 Cancri ..	5	8	49	1	+12	31
	76 κ Cancri	5·6		58	23	11	21
	Moon	(13)	9	12		10	45
	14 o Leonis	4		31	54	10	40
11	10 Leonis	5·6	9	28	3	+ 7	36
	32 α Leonis	1		59	12	12	48
	Moon	(14)	10	4		6	31
	23 h Sextantis ..	6		12	6	3	9
12	55 Leonis	6	16	46	48	+ 1	39
	Moon	(15)		58		1	59
	69 Leonis	5·6	11	4	54	0	52
	91 ν Leonis	4·5		28	5	0	8
13	91 ν Leonis	4·5	11	28	5	+ 0	8
	Moon	(16)		51		− 3	4
	26 χ Virginis ..	6	12	30	19	− 7	2
14	26 χ Virginis ..	6	12	30	19	− 7	2
	40 ψ Virginis ..	5·6		45	21	8	36*
	Moon	(17)		46		7	53
	49 g Virginis....	5·6		58	50	9	49
	67 α Virginis....	1	13	16	8	10	15
19	*Moon*	(22)	17	50		−19	56
	19 δ Sagittarii ..	3·4	18	9	55	29	53
	22 λ Sagittarii ..	3·4		17	18	25	30
20	22 λ Sagittarii ..	3·4	18	17	18	−25	30
	34 σ Sagittarii ..	3		44	32	26	30
	Moon	(23)		52		18	35
21	*Moon*	(24)	19	51		−16	3
	6 α^2 Capric.	3	20	8	27	13	4
	9 β Capric.	3·4		11	17	15	19
22	6 α^2 Capric.	3	20	8	27	−13	4

1827.	Stars.	Mag.	Æ. h	m	s	D. °	′
Mar. 22	β Capric.	3·4	20	11	17	−15	19
	Moon	(25)		48		12	33
April 5	1 Cancri	6	7	47	9	+16	15
	Moon	(9)		57		15	32
	27 e Cancri	6·7	8	17	9	13	13
	65 α^2 Cancri	5		49	1	12	31
6	50 A² Cancri	6	8	37	26	+12	44
	Moon	(10)		48		12	21
	65 α^2 Cancri	5		49	1	12	31
	76 κ Cancri	5·6		58	23	11	21
7	5 ξ Leonis......	5	9	22	37	+12	3
	14 o Leonis	4		31	54	10	40
	Moon	(11)		40		8	28
	29 π Leonis	4·5		51	4	8	52
	32 α Leonis	1		59	12	12	48
8	16 Sextantis	6	10	0	10	+ 7	0
	32 x Sextantis ..	7		23	19	5	31
	Moon	(12)		31		3	53
	36 n Sextantis ..	6		36	14	3	13*
	55 Leonis	6		46	48	1	39
9	69 Leonis	5·6	11	4	54	+ 0	52
	87 e Leonis	4·5		21	29	− 2	1
	Moon	(13)		24		− 0	45
	91 υ Leonis	4·5		28	5	+ 0	8
	(126) Virginis ..	7		29	34	− 1	29*
10	*Moon*	(14)	12	19		− 5	38
	26 χ Virginis ..	6		30	19	7	2
	Jupiter	....		33		1	46
11	40 ψ Virginis ..	5·6	12	45	21	− 8	36
	Moon	(15)	13	16		10	20
	67 α Virginis....	1		16	8	10	15
	86 Virginis	6		36	44	11	33
12	100 λ Virginis ..	4	14	9	46	−12	34
	Moon	(16)		18		14	26
	9 α^2 Libræ	3		41	23	15	19*
	24 ι^1 Libræ	5·6	15	2	22	19	8
13	24 ι^1 Libræ	5·6	15	2	22	−19	8
	(19) Scorpii	6·7		6	22	21	45
	28 Libræ	6		11	6	17	31
	Moon	(17)		20		17	36
14	8 β^1 Scorpii	2	15	55	24	−19	19
	Moon	(18)	16	25	40	19	30
17	*Moon*	(21)	19	33		−16	46
	6 α^2 Capric.	3	20	8	27	13	4
	9 β Capric.	3·4		11	17	15	19
18	2 α Capric.	3	20	8	27	−13	4
	β Capric.	3·4		11	17	15	19
	Moon	(22)		32		13	32

1827.	Stars.	Mag.	Æ. h	m	s	D. °	′
April 19	*Moon*	(23)	21	27		− 9	35
	49 δ Capric.	3		37	29	16	54
	34 α Aquarii	3		56	53	1	9
20	34 α Aquarii	3	21	56	53	− 1	9
	48 γ Aquarii	3·4	22	12	43	2	15
	Moon	(24)		19		5	14
May 6	62 g Leonis	6	10	54	45	+ 0	55
	Moon	(11)		59		1	32
	69 Leonis	5·6	11	4	54	0	52*
	91 υ Leonis	4·5		28	5	0	8
7	91 υ Leonis	4·5	11	28	5	+ 0	8
	Moon	(12)		51		− 3	15
	Jupiter	....	12	22		− 0	44
	26 χ Virginis ..	6		30	19	− 7	2
8	26 χ Virginis ..	6	12	30	19	− 7	2
	40 ψ Virginis ..	5·6		45	21	8	36*
	Moon	(13)		47		8	0
	49 g Virginis....	5·6		58	50	9	49
	67 α Virginis....	1	13	16	8	10	15
9	67 α Virginis....	1	13	16	8	−10	15
	86 Virginis	6		36	44	11	33
	Moon	(14)		44		12	26
	100 λ Virginis ..	4	14	9	46	12	34
10	100 λ Virginis ..	4	14	9	46	−12	34
	9 α² Libræ......	3		41	23	15	19
	Moon	(15)		47		16	10
	24 ι¹ Libræ	5·6	15	2	19	19	8
11	43 κ Libræ	5	15	32	0	−19	7
	45 λ Libræ	5		43	18	19	38*
	Moon	(16)		54		18	46
	8 β¹ Scorpii	2		55	24	19	19
	14 ν Scorpii	4	16	1	57	19	0
	4 ψ Ophiuchi ..	5		14	0	19	37*
12	18 Ophiuchi	6	16	39	14	−24	20
	(214) Scorpii ..	6·7		43	13	20	7
	(251) Ophiuchi..	7		49	40	17	58
	Moon	(17)		59		19	56
16	*Moon*	(21)	21	8		−10	55
	22 β Aquarii	3		22	27	6	20
	40 γ Capricorni	3·4		30	29	17	26
17	34 α Aquarii	3	21	56	53	− 1	9
	Moon	(22)	22	3		6	40
	48 γ Aquarii	3·4		12	43	2	15
18	34 α Aquarii	3	21	56	53	− 1	9
	48 γ Aquarii	3·4	22	12	43	2	15
	Moon	(23)		55		2	2

1827.	Stars.	Mag.	AR.			D.	
			h	m	s	°	′
May 19	54 α Pegasi	1	22	56	9	+14	17
	Moon	(24)	23	45		2	30
	88 γ Pegasi	2·3	0	4	20	14	13
20	57 α Pegasi	1	22	56	9	+14	17
	88 γ Pegasi	2·3	0	4	20	14	13
	Moon	(25)		34		6	49
June 5	40 ψ Virginis ..	5·6	12	45	21	− 8	36
	Moon	(11)	13	16		10	16
	67 α Virginis....	1		16	8	10	15
	86 Virginis......	6		36	44	11	33
6	100 λ Virginis ..	4	14	9	46	−12	34
	Moon	(12)		15		14	19
	9 α² Libræ	3		41	23	15	19*
7	24 ι¹ Libræ	5·6	15	2	19	−19	8
	Moon	(13)		17		17	32
	43 κ Libræ	5		32	0	19	7
	45 λ Libræ	5		43	18	19	38
8	8 β¹ Scorpii	2	15	55	24	−19	19
	4 ψ Ophiuchi ..	5	16	14	0	19	37
	Moon	(14)		23		19	31
	29 Ophiuchi	6		51	45	18	35
9	40 ρ Ophiuchi ..	4·5	17	10	38	−20	55
	Moon	(15)		33		19	59
	13 μ¹ Sagittarii..	3·4	18	3	25	21	6
10	13 μ¹ Sagittarii..	3·4	18	3	25	−21	6
	29 Sagittarii	6		39	24	20	32
	Moon	(16)		40		18	51
	43 d Sagittarii ..	5	19	7	30	19	18
14	34 α Aquarii ..	3	21	56	53	− 1	9
	48 γ Aquarii ..	3·4	22	12	43	2	15
	Moon	(20)		37		3	46
15	54 α Pegasi	1	22	56	9	+14	17
	Moon	(21)	23	28		0	54
	88 γ Pegasi	2·3	0	4	20	14	13
16	54 α Pegasi	1	22	56	9	+14	17
	88 γ Pegasi	2·3	0	4	20	14	13
	Moon	(22)		18		5	22
17	*Moon*	(23)	1	8		+ 9	29
	6 β Arietis	3		45	5	19	58
	13 α Arietis	3		57	26	22	39
18	6 β Arietis	3	1	45	5	+19	58
	Moon	(24)		57		13	5
	86 γ Ceti	3	2	34	21	2	30
19	6 β Arietis	3	1	45	5	+19	58
	13 α Arietis	3		57	26	22	30
	Moon	(25)		47		16	2

XVI. *Obser-*

XVI. *Observations on the late Solar Eclipse.* By THOMAS SQUIRE, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE day with us was rather unfavourable for observing the late solar eclipse; I could not see the beginning, as the sun was obscured by clouds (\– and ⌒–) at the time. But at about two minutes after 10 M. S. T. the sun became visible through the passing nascent *cumuli*, when the obscuration was very considerable on the north-west part of the sun's disc. The eclipse continued to be visible at intervals till near the middle, when a dense *cumulostratus* again obscured the sun, but towards the end the air became clear, and continued so till the termination of the eclipse; and which took place here at $12^h\ 0^m\ 54^s$ mean solar time. Latitude of the place $51°\ 41'\ 41''·6$ north. Longitude 27 seconds in time east of Greenwich. The above time reduced to that at the Royal Observatory gives $12^h\ 0^m\ 27^s$. Probably the end here was absolutely rather later than at Greenwich, owing to the effects of the lunar parallax, the moon being a little more depressed from our northern situation.

The above observations were made with one of Dollond's achromatic telescopes, and power of 80. The time was deduced from correct altitudes of the sun, taken with an excellent reflecting circle made by Troughton, having at the same time the latitude of the place and sun's declination given.

I remain, Gentlemen, yours truly,

Epping, Dec. 15, 1826. THOMAS SQUIRE.

XVII. *On Fustic* (Morus tinctorius), *and its Application to the Dyeing of Yellow, Green, Olive, and Brown.* By E. S. GEORGE, Esq. *F.L.S.**

THE wood of the *Morus tinctorius* is employed in dyeing those shades of yellow in which intensity of colour is of more importance than brilliancy, and in all the range of colours formed by the mixture of yellow, blue, and red.

For those colours in which the sulphate of indigo is employed to give the blue, it is of great value, resisting the action of free sulphuric acid in a higher degree than any other yellow colouring matter.

* Communicated by the Author.

Having

Having ascertained the chemical composition of this wood by some preliminary experiments upon 200 grains of fustic reduced to a fine powder and dried at 212° Fahr., poured 16 ounces of boiling water, left to digest till cool, decanted off the clear infusion, and repeated the digestions in 16 ounces of boiling water three times, poured the whole together and filtered, washed the filter with 16 ounces of water at 150° Fahr. added the washings to the filtered liquid, and evaporated the whole to dryness at a temperature not exceeding 160° Fahr. The dry mass weighed 30·10 grains. The insoluble part remaining upon the filter weighed 168·75 grains.

Upon the residual 168·75 grains after the action of water poured 6 ounces of boiling alcohol, and digested 24 hours; digested a second time in 6 ounces of alcohol, filtered, washed the filter with 2 ounces of alcohol, evaporated the alcoholic solutions (which were of a dark orange colour) to dryness, the residue, weighing 18 grains, had a shining resinous appearance, its colour black when seen in mass, and of a deep orange when finely divided: at a temperature of 300° Fahr. it melted.

Upon 100 grains of fustic in powder, and dried at 212° Fahr. were boiled 6 ounces of alcohol in a covered vessel one hour, poured off the solution which was of a dark orange colour, and again digested in 4 ounces of boiling alcohol half an hour, filtered both solutions and washed the filter with alcohol, evaporated the solution to dryness; the dry mass weighed 24 grains; digested the part remaining upon the filter in boiling water and evaporated the clear solution to dryness: a substance agreeing in all its characters with gum remained, it weighed 2 grains.

The residual woody fibre after the action of alcohol and water, weighed when dried at 212° Fahr. 74 grains.

To estimate the amount of tannin in the aqueous solutions, I first made some experiments to ascertain the proportions in which the peculiar tannin of fustic and gelatine combined. Having made a clear infusion of fustic containing 52 grains of aqueous extract, solution of isinglass was added gradually, as long as any precipitate fell down, the tannate was precipitated in large brown coloured flakes; found that 11 grains isinglass were required to throw down the whole, and that the tannate of gelatine formed weighed 25·30 grains; hence, it is composed of tannin 14·30, gelatine 11; or in 100,—tannin 56·53, gelatine 43·47.

To ascertain the amount of tannin, made an aqueous extract of the soluble matter contained in 200 grains of fustic, added solution of isinglass as long as any precipitate fell down. After being dried at 212° the tannate formed weighed 14 grains, containing 7·91 grains of tannin, or 3·95 per cent on the

the fustic examined. The solution from which the tannin had been separated gave a dark olive precipitate with solutions of the salts of iron, and a copious yellow one with the solutions of tin;—it consisted of colouring matter and gallic acid. In a former experiment the amount of aqueous extract was 15·05 per cent: after deducting 5·95 grains of tannin and gum, there remains 9·10 grains of gallic acid and colouring matter.

100 grains of fustic are composed of

Woody fibre	74
Resin	9
Gum	2
Tannin	3·95
Colouring matter and gallic acid	9·10
Loss	1·95
	100·00

The large amount of loss is probably occasioned by the great difficulty in bringing substances, which attract moisture so rapidly as woody fibre, to the same hygrometric state.

On the applications of Fustic.

The colouring matter of fustic is seldom employed in the dyeing of yellow: the only case in which it is so applied is as a cheap substitute for weld or quercitron; but for woollen goods intended to be dyed a true green in the indigo vat, the required shade of yellow is first given by means of fustic.

The dyeing vessel may be of iron; and for 120 yards of woollen cloth, weighing 1lb. 4oz. to the yard, 45lbs. of fustic in chips with 6lbs. of alum will be found sufficient for ordinary shades of green. If the shade required be bright, 4lbs. of solution of tin may be added with advantage, but for bottle-green an additional proportion of fustic will be required: some dyers use the fustic alone without any mordant, and the affinity of woollen fibre for the colouring matter of fustic is sufficiently powerful to fix the whole; the addition of a mordant, however, gives much greater durability. After the fustic and alum have been boiled a few minutes in a dyeing vessel containing from 300 to 400 gallons of water, 20 gallons of cold water are added and the cloths entered, turning quickly a few minutes and afterwards more slowly, and boiled from fifty minutes to an hour. They are afterwards well washed, and the requisite shade of blue given in the indigo vat.

Fustic is employed in all the shades known as Saxon green. In this class of colours the blue is obtained from indigo, but by means of its solution in sulphuric acid known by dyers as

Chemic. The Annals of Philosophy contain an interesting set of experiments made upon this combination by Mr. Crum. I shall only state that the long list of substances employed by the old dyers and chemists in making this solution are almost entirely discarded, and sulphuric acid and indigo are the only substances now employed. It is of great importance that the sulphuric acid should be free from nitrous gas, which by its action upon the indigo (deoxidizing) deprives the colour produced of brightness and lustre. In making the solution of indigo for greens an excess of sulphuric acid should be avoided, as it prevents the yellow colouring matter fixing upon the cloth. I have found 9lbs of sulphuric acid to 1lb. of indigo of good quality the best proportion.

For the dyeing of 100lbs. of worsted goods, known as Wildbores, a bright green. In a leaden vessel containing 300 gallons of water—when at the temperature of 150° Fahrenheit, threw in 25lbs of alum and 2 quarts of bran—carefully removed the impurities that rose to the surface until the water boiled, then added 2½ pints of sulphate of indigo; 12lbs. of fustic in chips, and 10lbs. of white Florence argol (super-tartrate of potash); boiled the whole five minutes, added 20 gallons of cold water, and entered the goods, turning quickly for ten minutes and then more slowly, at the same time raising the temperature to ebullition. After boiling forty-five minutes found the colour scarcely so full as required, and took out the goods, adding half a pint of sulphate of indigo and 4lbs. of fustic, again entered and boiled half an hour. Fresh goods may be dyed in the same liquid; indeed, in conducting a dyehouse œconomically, it is of great consequence so to arrange the colours that they shall follow each other without emptying the dyeing vessels, as thus a great saving of dyeing wares is achieved. For 100lbs. of the same description of goods, and the same shade of colour,—added 15lbs. of alum, 2½ pints of sulphate of indigo, and 7lbs. of argol: after entering and boiling as before 45 minutes, took out the goods, added half a pint of sulphate of indigo; entered and boiled twenty minutes. It is of importance that the whole of the indigo should not be given at first, since from the boiling necessary to give evenness to the colour the lustre is considerably impaired: by adding a part towards the close of the process both evenness and beauty of colour are insured. For a third quantity, the same colour, 12lbs. of alum were added, and the amount of alum in a fourth and fifth quantity must gradually diminish to 6lbs. The amount of fustic and argol are to be gradually reduced, the proportions depend, however, upon the discretion of the dyer; the proportion of sulphate of indigo remains the

same,

same, the whole of the blue colouring matter being removed from the dyeing vessel at each operation.

It is not advisable to continue more than six parcels of goods without emptying at least half the contents of the dyeing vessel, and filling with fresh water; but shades of olive or brown must succeed without any addition of water.

For all shades of olive and brown which may be considered as the same colour, only varying in the proportions of red, yellow or blue, entering into their composition; fustic is employed for the yellow, the blue is given by the sulphate of indigo, and for the red, madder is used for all the light shades of bronze approaching to green, and camwood for the darker shades of olive and brown.—I shall without further observation state some processes.

The light and green shades of bronze are generally dyed after green in the same liquor. For 126lbs. of worsted stuffs after light green, added 24lbs. of mull madder, 14lbs. of fustic in chips, 4lbs. of alum, 3lbs. of red argol, 2lbs. of sulphuric acid, and 1 pint of sulphate of indigo; boiled the whole together ten minutes, added 20 gallons of water, entered the goods turning quickly and afterwards more slowly, boiled one hour and thirty minutes, took out the goods and added 3 measured ounces of sulphate of indigo, entered and boiled thirty minutes. With olives, and indeed all the colours in which sulphate of indigo is employed except the very red browns, it is of consequence that a portion should be added towards the close of the operation, thus increasing the brilliancy of the blue part of the colour, which is impaired by the long boiling required to fix the yellow and red.

In the same manner are dyed all the shades of olive, the proportions varying with the colour required; the amount of mordant (alum) and acid employed, must diminish with the number of operations that have been conducted without emptying the dyeing vessel.

In dyeing the red shades of brown for which camwood is used, a different process is employed, the insoluble combination formed between its colouring matter and the base of alum prevents their being employed together.

For 90lbs. of worsted goods in fresh water dyed in a leaden vessel containing 300 gallons of water, added 15lbs of rasped camwood, 9lbs. of rasped fustic, 12 measured ounces of sulphate of indigo, 5lbs. of red argol, and 3lbs. of sulphuric acid; after boiling the whole together a few minutes, added 20 gallons of cold water, and entered and boiled 1 hour; the goods had acquired a dull red brown colour, took up, added 6lbs. of alum, and 8 measured ounces of sulphate of indigo, entered,

tered, and again boiled 1 hour; the colour thus obtained was a bright full red brown. In the same manner a similar shade of red brown, or others yellower, may be dyed in the same dyeing liquor, adding the alum after the red part of the colour has become fixed. After the above a yellow brown approaching to a snuff colour was dyed;—for 100lbs. of worsted goods added 2lbs. of camwood, 10lbs. of mull madder, 9lbs. of rasped fustic, 3lbs. of red argol, 14 measured ounces of sulphate of indigo, and 2lbs. of sulphuric acid,—boiled 1 hour. Took up, added 4lbs. of alum, 1lb. of sulphate of copper, 2lbs of rasped fustic, and 4 measured ounces of sulphate of indigo; entered and boiled 1 hour. A small portion of sulphate of copper increases the brilliancy and adds much to the intensity of the yellow browns.

The mode of dyeing olive and brown now described, has only been introduced to the dyeing establishments of this country since the last 20 years: it is called by dyers the sour way. The same colours, possessing however little brilliancy, were dyed with camwood, fustic, and logwood; the mordant employed was sulphate of iron.

For 59lbs. of a coarse woollen cloth called Calmuck, a full olive brown. Dyed in an iron pan containing 400 gallons of water; added 20lbs. of rasped fustic, 8lbs. of rasped camwood, 6lbs. of chipped logwood; boiled $1\frac{1}{2}$ hour: took up, emptied the dyeing vessel half-way, filled with fresh water, and added 2lbs. of sulphate of iron, entered the cloths turning quickly 10 minutes, raised gradually to ebullition, and boiled 10 minutes.

In the same manner may be dyed all the shades of copper, brown, and olive.

St. Peter's Hill, Leeds, Dec. 20th, 1826.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THURSDAY, being St. Andrew's day, the Royal Society held their Anniversary Meeting, at their apartments in Somerset House, for the election of Council, and Officers. As the award of the New Royal Medals and the Copley Medal was to be announced on this occasion, a great number of the Fellows were in early attendance.

The President took the chair at eleven o'clock, and began the business by reading the list of the Fellows who had been admitted, and those they had lost by death since the last anniversary. Among the foreign deceased members he mentioned, with particular notice, Scarpa, the celebrated anatomist of Pavia; and Piazzi, the dis-

coverer

coverer of the planet Ceres. These gentlemen, said the President, died according to the ordinary course of nature in old age, having enjoyed a glory which in no respect disturbed their repose. Among the home members, he dwelt at some length on the loss of Mr. Taylor Combe and Sir Stamford Raffles; the last of whom he eulogized as a most disinterested and liberal contributor to the Natural History of this country. "Occupying high situations," said the president, "in our empire in the East, he employed his talents and his extensive resources, not in the exercise of power or the accumulation of wealth, but in endeavouring to benefit and to improve the condition of the natives, to fix liberal institutions, and to establish a permanent commercial intercourse between the colonies in which he presided and the mother country; which, while it brought new treasures to Europe, tended to civilize and to improve the condition of the inhabitants of some of the most important islands of the East. Neither misfortune nor pecuniary losses damped the ardour of his mind in the pursuit of knowledge. Having lost one splendid collection by fire, he instantly commenced the formation of another; and having brought this to Europe, he made it not private, but public property, and placed it entirely at the disposition of a new Association for the promotion of zoology, of which he had been chosen president by acclamation. Many of the Fellows of this Society can bear testimony to his enlightened understanding, acute judgement, and accurate and multifarious information; and all of them must, I am sure, regret the premature loss of a man who had done so much, and from whom so much more was to be expected, and who was so truly estimable in all the relations of life."

After stating the foundation of the Royal Medals, which had been announced to the Society at their anniversary dinner last year by the Right Honourable the Secretary of State for the Home Department; and which, said the President, having been offered in the true spirit of Royal munificence, had been completed with an exalted liberality worthy of the august patron of the Royal Society—being intended to promote the objects and progress of science, by awakening honourable competition among the philosophers of all countries; he proceeded to state, that the council had awarded the first prize to Mr. John Dalton, of Manchester, for the development of the chemical theory of definite proportions, usually called the Atomic Theory, and for his various other labours in chemical and physical science. He mentioned the names of Dr. Bryan Higgins, Mr. William Higgins, and Richter, as having contributed something towards the foundations of this part of science; but, he said, as far as can be ascertained, Mr. Dalton was not acquainted with any of their publications; and whoever considers the original tone prevailing in all his views and speculations, will hardly accuse him of wilful plagiarism. But, let the merit of discovery be bestowed wherever it is due, and Mr. Dalton will be still pre-eminent in the History of the Theory of Definite Proportions. He first laid down clearly and numerically the doctrine of multiples, and endeavoured to express by simple numbers the weights of the bodies believed to be elementary. The first views, from their bold-

ness

ness and peculiarity, met with but little attention; but they were discussed and supported by Drs. Thomson and Wollaston, and the scale of chemical equivalents of the latter gentleman separates the practical part of the doctrine from the atomic or hypothetical part, and is worthy of the celebrated author. Gay-Lussac, Berzelius, Prout, and other chemists, have added to the evidence in favour of the essential part of Mr. Dalton's doctrine; and for the last ten years it has acquired, almost every month, additional weight and solidity.

The President begged to be understood, that it was the fundamental principle that he was contending for, and not Mr. Dalton's particular statement of the nature of bodies, and the numbers representing them, given in Mr. Dalton's New System of Chemical Philosophy. In this, he said, as a first sketch, many of the opinions are erroneous, and the results incorrect, and they are given with much more precision in later authors. It is in the nature of physical science that its methods offer only approximations to truth, and the first and most glorious inventors were often left behind by very inferior minds in the minutiæ of manipulation, and their errors enabled others to discover truth.

Mr. Dalton's permanent reputation, continued the President, will rest upon his having discovered a simple principle universally applicable to the facts of chemistry, in fixing the proportions in which bodies combine, and laying the foundation for future labours respecting the sublime and transcendental part of the science of corpuscular motion. His merits in this respect resemble those of Kepler in Astronomy. The causes of chemical change are as yet unknown, and the laws by which they are governed; but in their connection with electrical and magnetic phænomena, there is a gleam of light pointing to a new dawn in science. And may we not hope, said the President, that in another century, Chemistry having, as it were, passed under the dominion of the mathematical sciences, may find some happy genius, similar in intellectual power to the highest and immortal ornament of this Society, capable of unfolding its wonderful and mysterious laws. I trust, said the President, you will allow the justice of the decision of your council, which has claimed for our countryman the first testimony of Royal benevolence. There is, he said, another motive which influenced them, and which, I am sure, will command your sympathy. Mr. Dalton has been labouring for more than a quarter of a century with the most disinterested views. With the greatest modesty and simplicity of character, he has remained in the obscurity of the country, neither asking for approbation nor offering himself as an object of applause. He has but lately become a Fellow of this Society; and the only communication he has given to you is one, compared with his other works, of comparatively small interest. Their feeling, therefore, on the subject is perfectly pure. I am sure he will be gratified by this mark of your approbation of his long and painful labours. It will give a lustre to his character which it fully deserves. It will anticipate that opinion which posterity must form of his discoveries, and it may make his example more exciting to others

others in their search after useful knowledge and true glory. The president then announced, that the second medal on the Royal foundation was awarded to James Ivory, M.A., for his papers on the laws regulating the forms of the planets, on astronomical refractions, and on other mathematical illustrations of important parts of astronomy. He then entered into a particular view of the merits of the papers communicated by Mr. Ivory to the Royal Society, being seven in number, on the most difficult and abstruse points of science; and quoted M. de Laplace as having borne testimony to some of their merits. After paying some high compliments to Mr. Ivory for his disinterested pursuit of objects of science which have no immediate popularity, and which are intelligible only to a few superior minds, and stating that all the mathematicians of the council were unanimous in claiming this reward for him; he said he felt satisfaction in the hope that this reward might, as an example, renovate the activity of the Society, which for so many years was pre-eminent in this department of science, and that it might return, *veteris vestigia flammæ*, with new ardour to its long neglected fields of glory.

Whether, said the President, we consider the nature of the mathematical science or its results, it appears equally among the noblest objects of human pursuit and ambition. Arising a work of intellectual creation, from a few self-evident propositions on the nature of magnitudes and numbers, it is gradually formed into an instrument of *pure reason*, of the most refined logic, applying to and illustrating all the phænomena of nature and art, and embracing the whole system of the visible universe. And the same calculus measures and points out the application of labour, whether by animals or machines—determines the force of vapour, and confines the power of the most explosive agents in the steam engine—regulates the forms of structures best fitted to move through the waves—ascertains the strength of the chain bridge necessary to pass across arms of the ocean—fixes the principles of permanent foundations in the most rapid torrents; and, leaving the earth filled with monuments of its power, ascends to the stars, measures and weighs the sun and the planets, and determines the laws of their motions; and even brings under its dominion those cometary masses that are, as it were, strangers to us, wanderers in the immensity of space; and applies data gained from the contemplation of the sidereal heavens to measure and establish time, and movement, and magnitudes below.

In announcing the award of the medal on Sir Godfrey Copley's foundation, for this year, the President stated, that it had been given to James South, Esq. for his paper on the observations of the apparent distances and positions of 458 double and triple stars, published in the present volume of the Transactions.

The researches and observations of double stars, said the President, recommended by Mr. Mitchell, were pursued at first by Sir William Herschel, in the hope of discovering the parallax of the fixed stars; and, afterwards, when his discoveries opened new views of the nature of the sidereal heavens, with the hope of ascertaining whether

whether systems did not exist among the fixed stars bearing relation to the planetary world, and demonstrative of the laws of gravity. These researches, pursued for many years by Sir William Herschel, were after his death continued by his son, Mr. Herschel, and Mr. South, with an instrument adapted for the purpose by Mr. Troughton. The combined observations, according to Mr. South, establish several important points—such as occultations of fixed stars by each other, proper motions of fixed stars, and revolving systems, in which two stars perform to each other the office of sun and planet, several of them having revolutions which may be assigned from 53 to 1400 years. After giving a pretty extensive account of Mr. South's conclusions, the President said: When the importance of an acquaintance with the position of the fixed stars in the heavens is considered, on the accurate knowledge of which all our data in refined astronomy, and many of those in practical navigation depend; and when the new and sublime views of the arrangements of Infinite Wisdom, in the starry heavens, resulting from these inquiries, are considered, the Society will, I am sure, approve of this vote of the council. Mr. South procured his instruments at a great expense, and employed them at home, and carried them abroad, trusting entirely to his own resources. He has pursued his favourite science in the most disinterested and liberal manner, and has communicated all his results to this Society. There is another reason which may almost be considered as personal. Whoever has seen the methods in which observations of this kind are conducted, must be aware of the extreme fatigue connected with them—of the watchful and sleepless nights that must be devoted to them—of the delicacy of manipulation they require—and of the sacrifices of ease and comfort they demand.

In presenting the medal to Mr. South, the President referred to it as the oldest mark of distinction which the Society had to offer, and which was more valuable from the illustrious names to which it had done honour, and the great and extraordinary advances in natural knowledge with which it had been connected. Receive it, he said (to use a metaphor taken from the Olympic games), as the honorary olive crown of this Society; and may it be a stimulus to induce you to pursue and persevere in these highly interesting astronomical researches, and to steadily apply your undivided attention to them, secure that posterity will confirm their utility, and that the glory resulting from them will be exalted by time.

The Society then proceeded to the election of the council and officers, when on examination, the following was found to be the state of the lists. Eleven members of the old council to remain members of the new council:—

Sir Humphry Davy, Bart. Pres.; John Barrow, Esq.; The Lord Bishop of Carlisle; D. Gilbert, Esq. V.P. Treasurer; John F. W. Herschel, Esq. M.A. Secretary; Sir Everard Home, Bart. V.P.; Captain Henry Kater; John Pond, Esq. A.R.; James South, Esq.; W. Hyde Wollaston, M.D. V.P.; T. Young, M.D. Foreign Secretary.

The

The members elected into the council:—

John Abernethy, Esq.; Charles Babbage, Esq. M.A.; Capt. Francis Beaufort, R.N.; Robert Brown, Esq.; John George Children, Esq.; Charles Hatchett, Esq.; A. B. Lambert, Esq.; William Viscount Lowther; George Pearson, M.D; William Prout, M.D.

The following were elected officers of the Society:—

President, Sir Humphry Davy, Bart.; *Treasurer*, D. Gilbert, Esq. V.P.; *Secretaries*, J. F. W. Herschel, Esq. M.A., John G. Children, Esq.

The Society then adjourned to dine together at the Crown and Anchor Tavern.

Dec. 7.—A paper was read On the composition of James's Powder and of Pulvis Antimonialis; by J. Davy, M.D. F R.S.

Dec. 14.—On the relative powers of various metallic substances regarded as conductors of electricity; by W. S. Harris, Esq.: communicated by John Knowles, Esq. F.R.S.

Dec. 21.—On an improved differential thermometer; by A. Ritchie, M.A., Rector of the Academy of Tain: communicated by Sir H. Davy, P.R.S.

The Society then adjourned over the Christmas vacation, to meet again on Thursday, Jan. 11, 1827.

LINNÆAN SOCIETY.

Dec. 19.—A. B. Lambert, Esq. in the Chair:—A vacancy for ten Foreign Members was declared; and a certificate was presented recommending Charles Lucien Buonapartè, Prince of Musignano, author of several valuable works on American Ornithology, to fill one of the vacancies.

The continuation was read of Mr. W. S. MacLeay's "Remarks on the comparative anatomy of certain Birds of Cuba, with a view to their respective places in the system of Nature, or to their relations with other animals."

After insisting on the importance of studying Natural Arrangement and Comparative Anatomy in connexion with each other, in order to investigate, whilst examining particular organs, the place held in nature by the animals to which they belong, Mr. MacLeay proceeds to examine the principles of arrangement laid down by Aristotle, with reference to the plan of studying the variation of structure in different animals, in preference to classing them together according to an arbitrary division of organs. He then states, that on the appearance of Mr. Vigors's View of Ornithology, he naturally became anxious to know whether the affinities therein stated held good throughout, and on his arrival in Cuba he resolved to examine anatomically those forms, which from being Extra-European had hitherto been little studied. He prefaces his observations upon them with some remarks on the affinities of Vertebrata, and the Comparative Anatomy of Birds in general.

GEOLOGICAL SOCIETY.

Nov. 17th.—"A notice was read On some beds associated with the magnesian limestone, and on some fossil fish found in them," by the Rev. Adam Sedgwick, Woodwardian Professor, F.G.S.

This notice professes to be an abstract of a longer paper hereafter to be presented to the Society. (1.) It first describes a deposit which extends through Yorkshire and Durham and separates the magnesian limestone from the coal measures. It is principally composed of sand and sandstone: but in one or two instances red marl and gypsum have been found associated with it. Its general character in Yorkshire is intermediate between the gritstone of the carboniferous order, and the harder beds of the new-red-sandstone.

In the county of Durham it is said to appear in the form of a yellow incoherent sand of very variable thickness, which throws very great difficulties in the way of all mining operations within the limits of the magnesian limestone. On a great scale it is considered unconformable to the coal strata, and nearly co-extensive with the magnesian limestone; on which account it is classed with the latter formation. (2.) Next described is a deposit in some places of shell-limestone, alternating with variously coloured marl,—and in other places of thin-bedded, nearly compact limestone alternating with bituminous marls. In the county of Durham this deposit is associated with an extensive formation of marl slate. In this marl-slate many specimens of fish have been discovered; some of which appear to be identical in species with the fish in the marl-slate of Thuringia. In the same deposit have also been found many vegetable impressions. (3.) The great deposit of yellow magnesian limestone is briefly noticed; and it is said not uncommonly to exhibit traces of the muriates of lime and magnesia, a fact which is supposed to connect it with the new red-sandstone. (4.) The deposit of red marl and gypsum imbedded in the formation of the magnesian limestone is briefly described. (5) Lastly is noticed the deposit of thin-bedded limestone which surmounts the gypsum, and in which magnesia is not so uniformly diffused as in the inferior member of the formation. Traces of this deposit are said to have been discovered in the county of Durham. And in Yorkshire beds of galena have been found subordinate to it, and worked with advantage. (6.) Over these deposits comes the great formation of red marl and new-red-sandstone, which appears to be so intimately interlaced with the preceding subdivisions of the magnesian limestone, that the two formations cannot in any natural classification be separated from each other. The fossils found in various parts of the magnesian limestone are noticed, and are supposed to form a suite which more nearly resembles that of the carboniferous limestone than has generally been imagined.

A paper was read entitled "Observations on the bones of hyænas and other animals in the cavern of Lunel near Montpelier, and in the adjacent strata of *marine* formation," by the Rev. W. Buckland, D.D. Professor of Mineralogy and Geology, University of Oxford.

In a recent journey through France in the month of March 1826, the author visited the cave of Lunel near Montpelier, (to which his attention

attention had been drawn by the description of M. Marcel de Serres,) for the purpose of instituting a comparison between it and the caves in England previously described by himself; and the result has established nearly a perfect identity both in their animal and mineral contents, as well as in the history of their introduction.

The cave of Lunel is situated in compact *calcaire grossier*, with subordinate beds of globular calcareous concretions, and the whole of the rock having something of an oolitic structure. In working a free-stone quarry of this *calcaire grossier*, the side of the present cavern was accidentally laid open, and considerable excavations have since been made in it, at the expense of the French Government, for the purpose of extracting its animal remains that lie buried in mud and gravel, and of searching for the aperture through which all these extraneous substances have been introduced. These operations have exposed a long rectilinear vault of nearly 100 yards in length and of from ten to twelve feet in width and height. The floor is covered with a thick bed of diluvial mud and pebbles, occasionally reaching almost to the roof, and composed at one extremity chiefly of mud, whilst at the other end, pebbles predominate.

Some vertical fissures in another quarry of *calcaire grossier* a few miles distant, are filled with similar materials to those within the cavern, and containing occasionally a few bones, sometimes cemented by calcareous infiltrations to a breccia like that of Gibraltar, Cette, and Nice. These materials are similar in substance to, and are uninterruptedly connected with, a superficial bed of diluvium that covers the surface of these quarries, and are identical with the general mass of diluvial detritus of the neighbourhood.

Stalactite and stalagmite are of rare occurrence in the cavern of Lunel; hence neither its bones nor earthy contents are cemented into a breccia.

On examining the bones collected in the cavern by M. Marcel de Serres and his associate M. Cristol, Dr. Buckland found many of them to bear the marks of gnawing by the teeth of ossivorous animals; he also discovered in the cave an extraordinary abundance of balls of *album græcum* in the highest state of preservation. Both these circumstances, so important to establish the fact of the cave of Lunel having been inhabited, like that of Kirkdale, as a den of hyænas, had been overlooked by the gentlemen above mentioned. The more scanty occurrence of stalactite, and the greater supply of *album græcum* in this cavern than in those of England, (See *Reliquiæ Diluvianæ*, vol. i.) are referred to one and the same cause, viz. the introduction of less rain water by infiltration into this cave, than into that of Kirkdale; in the latter case a large proportion of the fecal balls of the hyænas appear to have been trod upon and crushed at the bottom of a wet and narrow cave, whilst at Lunel they have been preserved in consequence of the greater size and dryness of the chamber in which they were deposited.

M. Marcel de Serres has published a list of the animal remains contained in this cavern, which differ but little from those of Kirkdale: the most remarkable addition is that of the Beaver and the Badger, together with the smaller striped, or Abyssinian Hyæna.

 For

For these discoveries we are indebted to the exertions of M. Cristol, a young naturalist of Montpelier, whose observations on the geology of that district the author found to be in perfect accordance with his own.

With respect to the bones of Camels said to have been discovered in this cavern, Dr. Buckland found on comparing rigidly the only bone which was supposed to be of this animal with the proportions given in Cuvier, that it certainly does not belong to the Camel. In some few parts of the diluvial mud there occur the bones of Rabbits and Rats; and M. Cristol has also discovered the leg of a Domestic Cock. All these Dr. B. found on examination to be of recent origin (not adhering to the tongue when dry, as do the antediluvian bones). The Rats and Rabbits are supposed to have entered the cave spontaneously, and died in the holes which they had burrowed in the soft diluvial mud, and the Cock's bone to have been introduced by a Fox through a small hole in the side of the cavern, which had been long known as a retreat of Foxes, in the bottom of an ancient quarry.

Land shells, similar to those which hybernate in the soil, or fissures of the neighbouring rocks, are also found in the mud that filled the cave. The author considers that these may either be the shells of animals that in modern times have entered some small crevices in the side of the cavern to hybernate there, and have buried themselves in the mud; or that they entered in more ancient times, and died whilst the cave was inhabited by hyænas, and lay mixed with the bones before the introduction of the mud and pebbles, or that they were washed in by the same diluvial water which imported there the diluvial detritus in which they are now imbedded.

Dr. Buckland draws a strong line of distinction between the mud and gravel of the caves and fissures, which he considers to be part of the general diluvium so widely spread over the adjacent country, and the local freshwater formations occurring also in the same neighbourhood of Montpelier; and which differ as decidedly from them, and bear to them the same relation that the gravel on the summit of Headen Hill in the Isle of Wight, bears to the strata of freshwater limestone that lie beneath it.

The author next proceeds to consider the epoch of the deposition of the remains of quadrupeds that have been found in some extensive quarries of stone and sand in the Fauxbourg St. Dominique at Montpelier, imbedded in a very recent marine formation which has been described by M. Marcel de Serres, in the 4th volume of the Linn. Trans. of Paris.

In the central beds of this deposit, the remains of the Elephant, Rhinoceros, Hippopotamus, Mastodon, Ox and Stag, are found intermingled with those of Cetacea, Dugong, or Lamantin; they are more or less rolled, and are occasionally covered with marine shells. Beds of oysters also (the *Ostrea crassissima* of Lamarck,) and barnacles, occur in horizontal and nearly parallel strata amid the marine sand, and show this deposition to have taken place gradually and at successive though perhaps short intervals, rather than to have resulted from a sudden marine irruption. The period of this deposition

deposition is supposed by the author to have been that which immediately preceded, and was terminated by the last grand aqueous revolution which formed the diluvium.

To a similar and contemporaneous period with this upper marine formation of Montpelier, he refers the bones of the Elephant, Rhinoceros, &c. with marine shells, (oysters and barnacles adhering to them,) that have been found in certain parts of the Sub-apennine hills, and also the bones of similar quadrupeds and shells that occur in the Crag of Norfolk and Suffolk.

To the same period also he assigns the bones of the osseous breccia of Gibraltar, Cette, and other fissures and caves along the north coast of the Mediterranean; and the accumulation of the remains of bears, hyænas, &c. in the caves of Germany, England and France: he also attributes the same date to the bones of similar animals that are found buried in the sediments of the antediluvian freshwater lake of the Upper Val d'Arno.

Dec. 1.—An extract of a letter from B. de Basterol, Esq. to Dr. Fitton, V.P.G.S. was read.

The author gives a short account of the succession of the strata in the vicinity of Folkstone, about which there had existed some uncertainty; from whence it appears that the Folkstone marl (or Gault) is separated from the lowest beds of the chalk by a stratum of green-sand, and is itself succeeded by sand and stone also abounding in green particles. The order being as follows: 1st, white chalk; 2nd, gray chalk; 3d a) sand containing green particles, and indistinct organic remains. b) marl of a dirty white colour mixed with the sand, and containing compact nodules; 4th, the blue marl of Folkstone (Gault) with Hamites, Inocerami, Ammonites, and a small Belemnite. 5th, thick beds of sand and sandstone full of green particles, but void of organic remains.

The reading of a paper was commenced, entitled "Additional notes on part of the opposite coasts of France and England, including some account of the Lower Boulonnois, by Dr. Fitton, V.P.G.S."

ASTRONOMICAL SOCIETY.

Nov. 10.—There was read a letter addressed to the President by Lieut. Henry Foster, R.N., On the method of determining the longitude by moon-culminating stars. The method was employed in finding the longitude of Port Bowen, the station where the expedition for the discovery of a North-west passage, under the command of Capt. W. E. Parry, passed the winter of 1824-5. The observations were made with an excellent portable transit instrument by Dollond, of thirty inches focal length, and two inches aperture; and made as often as circumstances would admit, between Dec. 5, 1824, and April 1, 1825. The resulting longitude is $5^h\ 55^m\ 39^s{\cdot}2$ west of Greenwich; the latitude being $73^\circ\ 13'\ 39''{\cdot}4$ north.

During a residence of nine months at Port Bowen, Lieut. Foster had opportunities of trying most of the known methods for determining the longitude: that, by measuring the distance of the moon's limb

limb from a fixed star, he found from the peculiarities of the climate, to be subject to these inconveniencies: viz. The uncertainty in the amount of atmospherical refractions at moderate altitudes in extreme low temperatures:—The alteration of the index error from a change in figure of the instrument, caused by temperature during an observation, and the painful sensation of burning (denominated long ago by Virgil, the *scalding cold*) on touching intensely cold metal with the naked hand. In addition to which, the condensation of the vapour from the eye, in a thin film of ice on the eye-piece of the telescope, rendering the star and the moon's limb obscure:—and further, the absolute necessity of holding the breath during an observation, as well as when reading off the measured arc:—all of which are, evidently, serious obstacles to correct observation.

There was also read a communication from Dr. Rumker, of Stargard, Paramatta, to Dr. Gregory, containing an account of some observations made at the observatory there. This paper contains, 1st, Observations of the great comet in 1825, from October 18 to December 20, and the *elliptic* elements thence deduced, as follows:

Passage of perihelion Dec. .	$10^d\ 18^h\ 41^m\ 7^s$, M. T. Greenwich.
Long. of perihelion	318° 28′ 54″
—— of node	215, 44 58
Semiaxis major	27·78997
———— minor	8·227477
Sidereal revolution	53509·3 days
Inclination	33° 31′ 3″: motion retrograde.

2dly, Observations on the comet in Leo, 1825, from July 9 to 15th, and the resulting *parabolic* elements, viz.

Passage of perihelion May .	30·77265
Long. of perihelion	273° 4′ 37″
—— of node	200 17 34
Log. perihel. dist.	9·9552155
Inclination	58° 35′ 58″: motion retrograde.

3dly, Observations of the lunar eclipse, May 21, 1826, at Paramatta. Dr Rumker observed the immersions and emersions of about 30 spots, as well as the time of the beginning and the end, under very favourable circumstances. The darkness of the moon during its total obscuration was such, that the occultations of stars of the 8th and 9th magnitudes could distinctly be observed. Dr. R. only observed the occultation of a star of the 7th magnitude Immersion $12^h\ 34^m\ 38^s$: Emersion $12^h\ 48^m\ 41^s$ mean time. The declination of this star is 19° 46′ S. near λ ♎, which passed 7′ N. of the moon's limb. The star described a very small chord, immerging and emerging repeatedly behind the inequalities of the ☽'s disk before it finally disappeared.

Lastly, Observations of Mars, near his opposition, from May 5th to May 12, 1826, and the south polar distances of the planet, and his distances from α^2 ♎ in Æ and declination.

XIX. *In-*

XIX. *Intelligence and Miscellaneous Articles.*

SEPARATION OF ELAINE FROM OILS.

M. PÉCHET has proposed a new process for the above purpose, which is founded upon the property possessed by a strong solution of soda of saponifying stearine in the cold, without acting upon elaine. Shake the alkaline solution with the oil, then warm it slightly to separate the elaine from the soap of stearine; it is then passed through a cloth, and the elaine is then separated by decantation from the alkaline solution. This process always succeeds, except with rancid oils or such as have been heated.—*Ann. de Chim.*

SULPHURET OF CERIUM.

Dr. Mosander has succeeded in forming this compound by two different processes. 1st, When the vapour of sulphuret of carbon is passed over carbonate of cerium heated to redness, a sulphuret of cerium is obtained which resembles minium in appearance, but it is porous and light, and suffers no change either by exposure to air or water. 2dly, By fusing oxide of cerium with sulphuret of potassium (*de l'hépar*) in large proportion, at a white heat, and afterwards separating the hepar by water. The sulphuret of cerium remains in the form of small brilliant scales, resembling *aurum musivum* in powder; when examined with a lens they appear transparent and of a yellow colour.

These two kinds of sulphuret of cerium, differing in appearance, are readily dissolved by acids with the evolution of sulphuretted hydrogen gas, without any residuum of sulphur. The sulphuret of cerium is composed of 74 cerium, and 26 sulphur.—*Ibid.* Sept. 1826.

OXIDE OF CARBON.

M. Dumas has proposed the following method of preparing this gas: he mixes salt of sorrel with five or six times its weight of concentrated sulphuric acid; the mixture when heated in a proper apparatus yielded a considerable quantity of a gas composed of equal parts of carbonic acid gas and oxide of carbon; after absorbing the carbonic acid gas by potash, the oxide of carbon remains in a state of purity.

This result will be easily comprehended by supposing that the sulphuric acid seizes the potash and the water, and that the oxalic acid being incapable of existing under these circumstances, is resolved into carbonic acid and carbonic oxide.

This process may be successfully employed for examining the salt of sorrel of commerce. Bitartrate of potash treated in the same manner gives oxide of carbon, carbonic acid and sulphurous acid, and the liquor becomes black by the deposition of carbon. The salt of sorrel on the contrary, never yields sulphurous acid, and the sulphuric acid employed remains perfectly limpid and colourless.—*Ibid.* Sept. 1826.

ARTIFICIAL

ARTIFICIAL SULPHURET OF ZINC.

M. Berthier prepares this sulphuret as follows:—Dissolve zinc foil in sulphuric acid, and separate the small quantity of charcoal and lead which remains undissolved; evaporate the solution to dryness, and add a few drops of nitric acid to peroxidize the iron; calcine slightly to decompose a part of the sulphates, and redissolve in water. If the solution still contains iron, which may be determined by a prussiate, repeat the operation; when there is no iron remaining, add a few drops of hydrosulphuret of ammonia to separate any trace of lead which may be dissolved. By slowly heating the sulphuret in an earthenware crucible to whiteness, either alone or mixed with 15 per cent of charcoal, it is reduced to a sulphuret; but as it almost always happens that a portion of the sulphate is decomposed by the heat before charcoal can reduce it, the sulphuret is mixed with a little oxide; this may be separated by pure dilute muriatic acid, which readily dissolves the oxide, and acts but feebly upon the sulphuret; it is then to be washed and dried. The pure sulphuret of zinc is pulverulent, and as white as the oxide.—*Ann. de Chim.*

PROTOFERROCYANATE OF IRON.

It is not I believe generally known, that a solution of protoxide of iron without any admixture of peroxide, may be obtained by putting the metal into an aqueous solution of sulphurous acid, and suffering the mixture to remain for a short time without the contact of atmospheric air. When a solution of ferrocyanate of potash is added, a perfectly white precipitate is formed, which is the protoferrocyanate of iron. The action of sulphurous acid upon iron is also remarkable on another account, viz. that no gas is evolved during the solution of the metal, if made to take place in closely stopped bottles. It appears that a part of the sulphurous acid is decomposed by the nascent hydrogen of the water, and the sulphuretted hydrogen which results remains in solution.—R. P.

CYANIC ACID.

M. Liebig states that cyanic acid may be obtained in a separate state, by passing a current of sulphuretted hydrogen gas through water in which cyanate of silver is suspended. This acid reddens litmus strongly, its taste is acid; it possesses the smell which is always perceived when any of its salts are decomposed by an acid: it neutralizes bases perfectly, but when in contact with water it suffers decomposition in a few hours, and is converted into carbonic acid gas and ammonia. The sulphuretted hydrogen must not be passed so as to decompose all the cyanate of silver; for then the cyanic acid is converted into other products by the excess of sulphuretted hydrogen.—*Ann. de Chim.* Oct. 1826.

SEPARATION OF IRON FROM MANGANESE.

M. Quesneville, jun. proposes the following process for separating these metals:—Dissolve both oxides in muriatic acid and boil the solution for some time to expel all excess of acid, in order to render the solution as neutral as possible. Dilute the solution with a large quantity

quantity of water, and pass chlorine gas through it to peroxidize the iron entirely; then precipitate the liquor by arseniate of potash; the precipitate is of a greenish white colour, and consists entirely of arseniate of iron. After some hours filter the liquor and wash the precipitate with a large quantity of boiling water; dry it and calcine it strongly to obtain the oxide of iron; evaporate the solution which contains the arseniate of manganese almost to dryness, and add water to it; if there remain by accident any traces of arseniate of iron it separates. Then filter and decompose the solution by caustic potash, and the oxide of manganese when well washed is then perfectly pure.—*Journ. de Pharm.* Sept. 1826.

ACETATES OF MERCURY.

This salt, according to the experiments of M. Garot, consists of

Acetic acid	20·3
Protoxide of mercury . .	79·7
	100

Its theoretic composition, supposing it to be a neutral salt, he considers to be

Acetic acid	19·59
Protoxide of mercury . .	80·41
	100

The peracetate by experiment was found to consist of

Acetic acid	33
Peroxide of mercury . .	67
	100

And its theoretic composition is stated to be

Acetic acid	32
Peroxide of mercury . .	68
	100

Ibid. Sept. 1826.

PYRMONT HEAVY SPAR.

The heavy spar of Pyrmont has lately been analysed by Brandes and Gruner, with the following results: sp. gr. 3·942.

Sulphate of barytes	92·2
Sulphate of strontian	3·0
Sulphate of lime	0·5
Water	2·4
Oxide of iron, with a trace of manganese	0·2
Ferruginous silica and alumina . . .	0·8
	99·1

Another variety:

Sulphate of barytes	93·9
Sulphate of strontian	3·1
Sulphate of lime	0·5
Water	2·5
	100

Schweigger's Journal.

DISCOVERY OF A SUBSTANCE THAT INFLAMES UPON CONTACT WITH WATER.

The following details have been communicated to us, which it would be desirable to have verified. At Doulens, near Amiens, is a large manufactory for spinning cotton, which is lighted by oil-gas. This gas upon its return from the cast-iron cylinder, filled with red-hot coal, where it is formed, traverses a reservoir of oil in which it deposits a white liquid matter, which can be taken away by means of a spigot situated at the lower part of the reservoir. The workmen employed in this duty having dropped some of it to the ground upon water, the matter took fire spontaneously, and having run into a neighbouring rivulet, it spread itself upon the surface of the water, which appeared to be all on fire. The proprietor of the factory intends to send a bottle of this singular substance to M. Gay-Lussac, to have it chemically analysed.—*Bull. Univ.*

ENORMOUS FOSSIL VERTEBRA.

In the neighbourhood of Bridport, in Dorsetshire, a short time ago a labourer digging for an ingredient used in mortar, found a vertebra of an enormous animal, larger than that of the whale, and supposed to belong to a land animal. This curiosity is in the possession of a gentleman at Bridport, who generously rewarded the finder with ten guineas. Search has been made after the other parts of the same animal, but hitherto without success. The perforation for the spinal marrow is stated to be nearly equal in circumference to the body of a man.

AFRICAN EXPEDITION.

By the kindness of a friend we are enabled to lay before our readers the copy of a letter, addressed by the well known Captain Clapperton to one of his connexions in this quarter. It is dated from Hio, or Eyo, the capital of Youriba, 22d February 1826, and is highly interesting on many accounts:

"No doubt you, and all my other kind friends in our dear native land, would be much alarmed for my safety, when the sad news of the deaths of the rest of my party reached you, as bad news always travel fastest. I certainly was very ill when poor Pearce died; but the circumstance of having to act as my own doctor, and the powerful medicine I took, I believe saved me, not forgetting that Divine Power, which ever, when a man is plunged in deep distress, gives him new courage to exert himself, and bear up against all misfortunes. You may in some measure guess my feelings, when so many deaths occurred so rapidly in so small a party. It is impossible for me to express them. I may tell you how I acted when poor Pearce died, whose death affected me most. After closing his eyes, I sat before the corpse with my head between my knees for nearly an hour, without saying a word. I then ordered a light and a watch to be kept over the body, and crawled to the place where I had to pass the night, and next day saw him buried, and read the Church of England service over him. This was the most trying duty of all. It is little to see a man die; but to see the earth thrown on one whom you knew,

knew, loved, and revered when living,—the last, and best, and kindest of your companions,—that is indeed a burden. You may think it strange that I, a Presbyterian, should have read the service over the dead, but it is a good thing for the living. All my servants attended, as also the most respectable of the town's-people through Poyens. I have been well used here; and depart in two days for Youri, where poor Park was killed. I will get all his papers, if not sent home by Bello, and hear every circumstance connected with his death. I have made important discoveries here, as every foot is new ground. I have passed over a range of hills which were not known to exist before, and traversed one of the most extensive kingdoms in Africa, the very name of which was unknown to Europeans. In the capital of this kingdom I have remained upwards of two months. The celebrated Niger is only two days' journey to the eastward of me; its course to the sea in the Bight of Benin can be no longer doubtful. I would say much more in this letter, but copies of my journals, with all my observations, have to be sent home. I trust you will write by the way of Tripoli, as the western route is doubtful."—*Dumfries' Courier.*

STEAM NAVIGATION.

Whatever may be the result of the attempts now making to establish a communication between this country and Great Britain by steam-vessels, we congratulate our readers on the rapid progress made in the establishment of steam-navigation in this country. Besides the government vessel *Enterprize*, employed between this and Rangoon, we have the *Diana* in Rangoon river; and the *Comet*, one of the two small vessels here, of twenty-four horse power, fitted up as packets to proceed up or down the river with passengers, is found to answer extremely well. The other vessel of this description will also be ready in a few weeks, and both are, by their light draft of water, we understand, admirably adapted for carrying passengers to the Upper Provinces during the rains, when the rivers are full: they are elegant models, and their accommodations most spacious and well laid out, as they have poops, and thus have a complete suite of cabins above and below, so that two families can be accommodated with every convenience. Besides these vessels, for which we are indebted to the enterprizing spirit of private individuals, the two armed steam-vessels of government will be ready in August next. Singapore too will soon boast of a steam-vessel for the Cape, and ere long, doubtless, each of the presidencies will have one or two in the service of the Company; meanwhile we learn that depôts of coals are about to be provided at Madras, Ceylon, and Penang. There is yet another vessel in progress here to be worked by steam, to which we have not yet alluded: we mean the one to be employed to clear away the impediments which, during the dry season, choke the navigation of the small rivers communicating with the Hoogly. By this vessel it is hoped that the water communication with the Upper Provinces will be kept open at all seasons of the year, and then a trip up to the most distant stations, which has been hitherto a most formidable under-

 taking,

taking, and a voyage of four months, perhaps may, by the aid of such light steam-vessels as these we have been alluding to, be performed in two or three weeks. Surely, when we consider that it is not more than three years since the first steam-vessel was seen in the river Hoogly, and when we consider that nothing was done for a considerable time after her appearance towards the acceleration of steam-navigation in India, the actual state of it at present is a just subject for congratulation.—*Col. Press Gaz.* June 9.

SCIENTIFIC BOOKS.

Just Published.

General Directions for Collecting and Preserving Exotic Insects and Crustacea: designed for the use of residents in foreign countries, travellers, and gentlemen going abroad. With illustrative plates. By George Samouelle, A.L.S.

Preparing for Publication.

A New Edition of Meteorological Essays, by James Frederick Daniell, Esq. F.R.S. This edition,—besides the former Essays upon, I. The constitution of the atmosphere. II. The construction and uses of a new hygrometer. III. The radiation of heat in the atmosphere. IV. The horary oscillation of the barometer. V. The climate of London, with corrections and additions,—will comprise Essays upon the following subjects: VI. Evaporation as connected with atmospheric phænomena. VII. Artificial climate considered with regard to horticulture. VIII. The connexion between the oscillation of the barometer at distant places. IX. The insinuation of air into the Torricellian vacuum, and the means of preventing the gradual deterioration of barometers. It will also contain various meteorological observations and remarks, and numerous tables, plates, and diagrams.

On the 1st of February, with numerous engravings on wood, Dr. Arnott's Work on General and Medical Physics. It is a system of Natural and Experimental Philosophy, with strictly scientific arrangement, but made easily intelligible to those who have never learned, or who have forgotten the mathematics. In addition to a great mass of illustrations from general nature and the arts, adapted to the present more comprehensive scale of a liberal education, it comprises many very interesting particulars furnished by examination of the animal body under health, disease, and medical treatment; and among these are disquisitions and suggestions.

FOREIGN BOOKS OF SCIENCE LATELY PUBLISHED.

Memoire sur l'Impossibilité de quelques Equations indeterminées du 5[e] degré; par G. Lejeune Dirichlet.

Journal fur die reine und angewandte Mathematik, von M. A. L. Crelle.

Observations Astronomiques publiées par le Bureau des Longitudes de Paris.

Correspondence Mathematique et Physique; par MM. Garnier et Quetelet.

Sull' Applicazione de' Principii della Mecanica Analitica, &c; di Gabrio Piola.

A Geolo-

A Geological Survey of the Environs of Philadelphia. By M. Froost.

Su i valori delle Misure e dei Pesi degli antichi Romani, desunti dagli originali esistenti nel real Museo Borbonico di Napoli, &c.

Considérations sur la diversité des Bassins de differentes Races Humaines, par M. G. Vrolik, D M.

Nouvelles Règles sur l'art de Formuler; par H. Briand, D. M. &c.

Considérations chimiques et médicales sur l'Eau de Selters ou de Seltz naturelle; par M.M. Caventou, François, Gasc, et Marc.

NEW PATENTS.

To Thomas Machell, of Berners-street, Oxford-street, surgeon, for improvements on apparatus applicable to the burning of oil, &c. —Dated the 8th of December 1826.—6 months allowed to enrol specification.

To Robert Dickinson, of New Park-street, Southwark, for an invention for the formation, coating and covering of vessels or packages for containing, preserving, or conveying goods, whether liquid or solid, &c.—8th of December.—6 months.

To Charles Pearson, of Greenwich, esquire, Richard Witty, of Hanley, Staffordshire, engineer, and William Gillman, of Whitechapel, engineer, for a method of applying heat to certain useful purposes.—13th of December.—6 months.

To Charles Harsleben, of Great Ormond-street, esquire, for his machinery for facilitating the working of mines and extraction of diamonds, &c. gold, silver, &c. from the ore, the earth, or the sand; applicable likewise to other purposes.—13th of Dec.—6 months.

To John Costigin, of Collon, in the county of Louth, civil engineer, for improvements in steam machinery or apparatus.—13th of December.—6 months.

To Peter Mackay, of Great Union-street, Borough Road, for improvements by which the names of streets and other inscriptions will be rendered more durable and conspicuous.—13th of December.—6 months.

To William Johnson, of Droitwich, for improvements in the mode of process and form of apparatus for the manufacturing of salt and other purposes.—18th of December.—6 months.

To Maurice De Jough, of Warrington, cotton-spinner, for improvements in machinery or apparatus for preparing rovings, and for spinning and winding fibrous substances.—18th of December.—6 months.

To Charles Harsleben, of Great Ormond-street, esquire, for improvements in building ships and other vessels, applicable to various purposes for propelling the same.—20th of December.—6 months.

To Thomas Quarrill, of Peter's Hill, London, for improvements in the manufacture of lamps.—20th of December.—6 months.

To William Kingston, master millwright of Portsmouth Dock-yard, and George Stebbing, mathematical instrument-maker, of High-street, Portsmouth, for improvements on instruments or apparatus for the more readily or certainly ascertaining the time and stability of ships or other vessels.—20th of December.—6 months.

To

To Melvil Wilson, of Warnford-court, Throgmorton-street, for improvements in machinery for cleaning rice.--20th of Dec.—6 mo.

To Charles Seidler, of No. 1, Crawford-street, Portman-square, for a method of drawing water out of mines, wells, pits, and other places.—20th of December.—6 months.

To Frederick Andrews, of Stanford Rivers, Essex, for improvements in the construction of carriages and in the engines or machinery to propel the same, to be operated upon by steam or other suitable power.—20th of December.—6 months.

To Charles Random Baron de Barenza, of Target Cottage, Kentish Town, for improvements in gunpowder-flasks, powder-horns, or other utensils of different shapes, such as are used for carrying gunpowder, in order to load therefrom guns, pistols, and other fire-arms.—20th of December.—6 months.

To Valentine Bartholomew, of Great Marlborough-street, for his improvement in shades for lamps, &c.—21st of Dec.—2 months.

To John Gregory Hancock, of Birmingham, plated beading and canister hinge manufacturer, for a new elastic rod for umbrellas and other the like purposes.—21st of December.—2 months.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1826.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·40 Nov. 21. Wind N.E.—Min. 28·60 Nov. 13. Wind N.
Range of the mercury 1·80.

Mean barometrical pressure for the month	29·776
——— for the lunar period ending the 28th instant	29·811
——— for 15 days with the Moon in North declination . .	29·894
——— for 14 days with the Moon in South declination . .	29·728
Spaces described by the rising and falling of the mercury . . .	7·980

Greatest variation in 24 hours 0·900.—Number of changes 17.

Therm. Max. 59° Nov. 12. Wind S.W.—Min. 29° Nov. 26. Wind N.W.
Range 30°.—Mean temp. of exter. air 44·88°. For 30 days with ☉ in ♎ 47·02.
Max. var. in 24 hours 20°·00—Mean temp. of spring water at 8 A.M. 54°·30.

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 13th . . .	98·00°
Greatest dryness of the air in the afternoon of the 8th	49·00
Range of the index	49·00

Mean at 2 P.M. 65·7—Mean at 8 A.M. 73·2—Mean at 8 P.M. 75·2
—— of three observations each day at 8, 2, and 8 o'clock . . 71·4
Evaporation for the month 1·15 inch
Rain near ground 3·640 inches.—Rain 23 feet high 3·410 inches.
Prevailing wind, North.

Summary of the Weather.

A clear sky, 5; fine, with various modifications of clouds, $10\frac{1}{2}$; an overcast sky without rain, $7\frac{1}{2}$; foggy, $\frac{1}{2}$; rain, $6\frac{1}{2}$.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
17	7	29	0	17	16	17

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
8	5	1	1	1	4	3	7	30

General

General Observations.—Considering that November is, in general, the most wet and gloomy month in the year, we may term the present a fine month; but the prevailing northerly and easterly winds made it cold. It seldom happens in November that we can see the cœrulean hue of the sky five days without clouds, which was the case this month. But during one-third of the month much low haze and vapours prevailed, which, from the obliquity of the sun's rays, had rather a gravitating than an ascending power. Their floating so near the earth's surface was the cause of excessive dampness for several days, which seemed to have a more powerful effect on the human constitution, than the cold weather with brisk drying winds had. On the penetrating effects of sudden vicissitudes of the weather in autumn, we may draw some inference from the discoloured or rusty state of the standing leaves of trees, plants, and polished metals, which is communicated to them by the superabundant quantity of oxygen in the atmosphere near the earth. Nearly two-thirds of the month we have had hoar frost on the ground in the mornings, sometimes accompanied by ice, which brought on the appearance of an early winter. So early as the middle of the month the hills in the northern districts were said to have been covered with snow.

On the 13th instant, there was a very sudden depression of the quicksilver in the Barometer, and more than an inch of rain fell in the 24 hours: after the quicksilver had attained a pretty high altitude, the dense clouds on the 19th, 20th, and 21st, presented a snowy appearance. The 26th was the coldest day here, when the Thermometer sank three degrees below the freezing point, and icy efflorescences appeared on the inside of the windows the first time this autumn, which indicated that it had frozen within doors.

The mean temperature of the external air this month is nearly three degrees lower than the mean of November for the last ten years.

In the morning of the 11th between 8 and 9 o'clock, three parhelia appeared in the South-east quarter; one on each side of, and the third above the sun, which latter was formed by the intersection of an inverted coloured arch with the solar halo that accompanied it, and each parhelion was 22° 35' distant from the sun's centre.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are three *parhelia*, one solar and one lunar halo, five meteors, two rainbows, sheet-lightning in the night of the 26th, and two gales of wind, namely, one from the North, the other from the S. W.

London.—Nov. 1. Fine. 2. Fine. 3. Fine day: rainy evening and night. 4, 5. Rainy. 6. Fine. 7. Fine: white frost. 8. White frost this morning: fine day. 9. White frost this morning: fine day. 10—12. Fine. 13. Cloudy day: rainy night. 14—16. Fine. 17. Cloudy. 18, 19. Rainy. 20. Fine. 21, 22. Cloudy. 23—25. Fine. 26. Hoar frost with dense fog: afternoon clear: some snow in the evening. 27. Foggy morning: afternoon fine: a little snow in the night. 28. Rainy. 29. Fine during the eclipse of the sun: rain in the afternoon. 30. Cloudy.

Penzance.—Nov. 1. Showers. 2, 3. Fair. 4—6. Fair: showers. 7. Showers. 8. Fair. 9, 10. Fair: showers. 11. Rain. 12. Showers. 13. Rain. 14. Hail showers. 15. Showers. 16. Rain. 17, 18. Showers. 19. Clear: fair. 20—23. Fair. 24. Showers. 25. Showers: hail and snow. 26. Fair. 27. Clear: hail shower. 28. Rain. 29, 30. Showers: rain at night.

Boston.—Nov. 1. Cloudy. 2, 3. Fine. 4. Rain. 5. Cloudy: rain A.M. and P.M. 6. Cloudy. 7, 8. Fine. 9. Cloudy. 10. Rain. 11. Cloudy. 12. Fine. 13. Fine: rain at night. 14. Rain. 15, 16. Fine. 17. Rain. 18. Cloudy. 19. Cloudy: rain A.M. 20—24. Cloudy. 25. Stormy. 26. Fine: snow P.M. 27. Fine. 28. Cloudy: rain P.M. 29. Cloudy. 30. Cloudy: rain A.M.

Meteor-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1826.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Gosp. 8½ A.M.	Barometer. Bost. 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Gosp. 8½ A.M.	Thermometer. Bost. 8½ A.M.	Hyg. Gosp.	Wind. Lond.	Wind. Penz.	Wind. Gosp.	Wind. Bost.	Evapor. Lond.	Evapor. Gosp.	Rain. Lond.	Rain. Penz.	Rain. Gosp.	Rain. Bost.
Nov. 1	29·96	29·94	29·71	29·70	29·80	29·35	50	38	50	57	49	46	67	NW.	NW.	W.	NW.	...	...	...	...	0·155	...
2	30·06	29·96	29·78	29·74	29·79	29·54	50	42	52	47	42	41·5	68	N.	N.	N.	NW.	...	...	...	...	...	...
3	30·06	29·95	29·76	29·76	29·87	29·70	52	47	55	44	45	44	70	N.	NE.	N.	NW.	...	0·15	·95	...	·250	...
4	30·05	29·95	29·76	29·68	29·73	29·55	47	39	54	48	49	50	84	N.	NE.	NE.	E.	...	...	·35	...	·160	...
5	30·04	29·97	29·71	29·70	29·87	29·60	50	43	54	48	48	51	78	N.	NE.	N.	NE.	...	...	·26	...	·050	0·63
☽ 6	30·10	29·97	29·72	29·70	29·78	29·40	48	27	54	48	48	45	77	NW.	N.	NW.	NE.	...	·10	...	...	·025	·22
7	30·27	30·10	29·92	29·80	29·90	29·66	42	30	48	36	33	34·5	58	NW.	N.	NW.	NW.	...	...	...	...	...	...
8	30·34	30·27	29·96	29·94	30·06	29·80	40	28	51	36	36	35	70	NW.	NW.	NW.	NW.	...	...	...	...	...	...
9	30·34	30·25	29·99	29·98	30·13	29·92	42	29	52	38	35	36	64	NW.	NW.	N.	NW.	...	·15	...	...	...	...
10	30·25	30·00	30·00	29·98	30·08	29·78	47	40	54	38	40	36·5	70	SW.	NW.	W.	NW.	...	...	...	...	·020	...
11	30·00	29·84	29·70	29·68	29·83	29·50	54	41	54	40	50	48	81	SW.	W.	SW.	NW.	...	...	...	...	·030	·20
12	29·84	29·65	29·49	29·41	29·66	29·30	54	33	54	47	47	41	78	SW.	W.	SW.	NW.	...	·10	·01	0·15	·010	...
13	29·65	29·25	29·10	28·70	29·40	29·06	48	33	55	46	48	38	78	SW.	SW.	SW.	NW.	...	...	·55	...	1·010	...
○ 14	29·77	29·25	29·50	29·10	29·10	28·80	44	38	52	44	41	39·5	76	NW.	N.	NW.	NW.	...	...	...	...	...	·24
15	30·18	29·77	29·70	29·62	29·56	29·26	48	28	52	46	42	38	69	NW.	NW.	NW.	NW.	...	·15	...	0·20	...	...
16	30·18	30·08	29·68	29·66	29·92	29·70	45	32	51	38	36	34·5	86	W.	SW.	N.	NW.	·47	...	·05	...	·250	...
17	30·33	30·08	29·74	29·73	29·82	29·67	47	37	50	44	47	44	73	SE.	W.	SW.	E.	...	...	...	...	·025	·16
18	30·33	30·33	29·92	29·91	30·06	29·90	47	44	50	38	45	43	75	NE.	W.	NE.	E.	...	·10	...	0·08	·050	...
19	30·54	30·33	30·00	29·92	30·08	30·	45	44	50	42	47	47	79	NE.	NE.	NE.	E.	...	...	·12	...	·010	·06
20	30·64	30·54	30·15	30·14	30·27	30·13	42	40	49	43	46	44	68	NE.	NE.	NE.	E.	...	...	...	...	...	...
21	30·64	30·58	30·22	30·20	30·40	30·25	45	37	48	43	44	43	70	NE.	NE.	NE.	E.	...	·15	...	...	...	...
☾ 22	30·58	30·28	30·20	30·[illegible]5	30·36	30·13	45	41	48	38	44	41	58	N.	NE.	N.	NE.	...	...	...	...	...	...
23	30·28	29·73	29·90	29·84	30·08	29·80	49	43	50	40	46	44	70	NW.	NW.	N.	NW.	...	...	...	...	·040	...
24	29·73	29·49	29·48	29·38	29·54	29·20	46	30	54	42	44	42·5	77	WNW	NW.	W.	calm	...	·10	...	...	...	...
25	29·53	29·36	29·10	29·08	29·19	28·76	41	20	40	34	39	36·5	60	W.	NW.	W.	W.	...	...	...	...	·015	...
26	29·92	29·53	29·30	29·16	29·26	29·10	35	25	40	36	32	29	68	WNW	NW.	N.	calm	...	...	...	...	...	...
27	29·99	29·92	29·60	29·60	29·63	29·46	37	30	50	36	30	27·5	74	W.	NW.	NW.	NW.	...	·05	...	...	...	...
28	30·00	29·60	29·60	29·50	29·68	29·52	51	38	54	36	41	34	80	SW.	W.	SW.	calm	...	...	·33	...	·600	...
● 29	29·60	29·59	29·18	29·16	29·38	29·10	51	40	54	46	50	47·5	86	W.	W.	SW.	calm	...	...	·09	...	·940	·36
30	29·68	29·60	29·30	29·28	29·33	29·15	45	37	50	45	47	40	85	S.	NW.	S.	calm	·38	·10	·01	1·700	...	·03
Aver.:	30·64	29·25	29·71	29·64	29·785	29·53	54	20	51	42	43·03	40·7	73·2					·85	1·15	2·72	1·743	3·640	1·90

THE

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[NEW SERIES.]

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XX. *On some new auxiliary Tables for determining the apparent Places of the Greenwich Stars.* By FRANCIS BAILY, *Esq. F.R.S. L.S. and G.S. and M.R.I.A.*

To Mr. Taylor.

1. IN a late Number of your valuable Journal*, you have noticed the *New Tables for facilitating the computation of Precession, Aberration and Nutation* recently published by the Astronomical Society of London. These tables certainly present the most convenient mode of computing those quantities, when *occasionally* required: but they, by no means, supersede the utility and (in the present state of astronomy, I may say) the *necessity* of those *Special Tables* for the *daily* corrections of the Greenwich stars, which were computed also at the expense of the Astronomical Society, at the suggestion (I believe) of Mr. Herschel; and who has written an excellent Introduction to the same.

2. It is well known that Mr. Herschel's tables are intended only to be *subsidiary* to the formation of other tables of the *corrections* which are *daily* required in an active observatory: and to save the time and labour which must otherwise be employed in determining those quantities. And, in the computation of those daily values, it will be evident to the experienced calculator that a still greater saving of time and labour might be effected if the computations of one year could be made subservient to those of the following years. This may be readily done with respect to the *aberration:* since the place of the sun on any given day will never differ more than 15 or 16 minutes from its place on the same day in any contiguous year: and by a previous arrangement of the tables, the cor-

* Phil. Mag. Nov. 1826.

rection for such difference may be easily applied. The *solar-nutation* likewise, depending on twice the same argument as the aberration, may be conveniently added thereto: and the *precession*, being a constant quantity, and proportional to the time clapsed, may also be easily united to the aberration. It follows therefore that tables of *precession*, *aberration* and *solar-nutation* for *each* Greenwich star may be so formed, for every tenth day of the year, as to last for many years to come, without the necessity of any other correction than such as arises from the position of the sun on the given day: and the practical astronomer may at any time take out, almost by inspection, the proper quantities required. With respect to the *lunar-nutation*, (to which I shall presently refer,) its value will differ from year to year so considerably, that *annual* computations must be made for each year, and for each star. But, since four computations for each star, in every year, will be sufficient, they may be so arranged from year to year as to be united with the daily values above alluded to. If Mr. Herschel's tables, therefore, be considered as the *first step* towards the formation of these daily corrections of the stars, I think that Tables, constructed agreeably to the arrangement here alluded to, may be fairly considered as the *second step* towards that desirable object.

3. The subject of the present communication is to point out the best mode of making this arrangement: and in doing this, I must again advert (as I have so frequently done before) to the valuable labours of M. Bessel, who has, in his *Fundamenta Astronomiæ*, page 67, shown the principle upon which this arrangement is made. But, as a more detailed explanation of it may be acceptable to many of your readers, and may probably lead to a more extensive adoption of it in this country, I hope I need not apologize for the space which I must necessarily occupy for this purpose in your interesting Journal.

4. Special tables, for determining the apparent places of the Greenwich stars, are generally so constructed as to show the position of the given star at the moment of its *culmination:* that being the time at which these stars are more usually observed. And this is the method which I shall adopt in the subsequent investigation*. Now, in order to deduce the values of the corrections from one year to another, a *fictitious year* must be assumed, commencing at a given epoch; and consisting of 366¼ *sidereal* days. M. Bessel has assumed the epoch of the sun's mean longitude at mean noon at *Paris*, on

* In the Nautical Almanac, however, the apparent places are given for *noon*.

January

January 0, 1800, being exactly 280°: but, in order to preserve uniformity in the arrangement of the tables published by the Astronomical Society, I shall assume the epoch of the sun's mean longitude at mean noon at *Greenwich* on January 1, 1800, being exactly 281°: which, in fact, differs very little from the preceding assumption. The mean motion of the sun in a *sidereal* day is 58′ 58″,6417; which, in 10 such days, amounts to 9° 49′ 46″,417: and it will be seen that the intervals of computation may be extended to 10 days, without any risk of error.

5. This being premised, we shall find that the mean longitude of the sun, when any given star culminates, on any given day of the fictitious year, will be equal to

$$280^\circ\ 13'\ 57'',88 + (d + \alpha)\ 58'\ 58'',6417$$

where α denotes the right ascension of the given star (in time), expressed in the fractional part of a day (or 24^h): and d the number of sidereal days, reckoned from the given epoch to the given day*. Consequently the mean longitude of the sun for the moment of culmination of any given star for every 10th day of the fictitious year will be†

$$280^\circ\ 13'\ 57'',88 + \alpha\ 58'\ 58'',64 +$$

for January 1	=	0° 0′ 0″0	
11	=	9 49 46 ,4	
21	=	19 39 32 ,8	
31	=	29 29 19 ,2	
February 10	=	39 19 5 ,6	
&c.		&c. &c. &c.	

The sun's *mean* longitude for each star, being found in the manner thus described for every tenth sidereal day, we must apply the *equation of the centre*, in order to determine the corresponding *true* longitude of the sun at the same periods; which will be the Argument for finding the aberration. And twice this quantity will be the Argument for finding the solar-nutation.

6. But we may readily form a table o the *true* longitude of the sun corresponding to every degree of his *mean* longitude in the fictitious year; which will last for many years to come.

* For those stars, therefore, whose right ascension is between 0^h and $18^h\ 44^m$, the time of culmination, will refer to the *preceding* day.

† As an example, take the case of α *Aquilæ*, the mean right ascension of which star, for January 1, 1830, is $19^h\ 42^m\ 29^s = \cdot 821$: and the mean longitude of the sun at the time of its culmination on that day is consequently 280° 13′ 58″ + 48′ 26″ = 281° 2′ 24″. This value being added to 9° 49′ 46″, and its multiples, will give the mean longitude of the sun at the time of its culmination on every subsequent tenth *sidereal* day of the year.

come. For, since the place of the sun's perigee in 1830 will be exactly 280°, and since it varies only 62″ from year to year, a table of the *equation of the centre* computed for the first 90° of the sun's mean anomaly will answer for the whole circle: attention being paid to the signs. Let z denote the mean anomaly of the sun for the year 1830, then will the equation of the centre be

$$+ 1^\circ\ 55'\ 22'',81 \sin z + 72'',61 \sin 2z + 1'',06 \sin 3z$$

7. Having thus determined the *true* longitude of the sun, for each star, on every *tenth* sidereal day of the fictitious year, we much enter Mr. Herschel's tables with the Arguments

$$(\odot + \mathrm{N}) \text{ and } (2\odot + \mathrm{N}'')$$

and find the respective values of the quantities required. The values of N and N″ are given by Mr. Herschel*.

8. With respect to the precession, or rather the *annual variation*, its amount at any given moment of time will be expressed by

$$\mathrm{V} \times \frac{\mathrm{L} - 281^\circ}{360^{\circ\prime}}$$

where V denotes the *annual variation*, and L the *mean* longitude of the sun as above determined†. If we substitute the value of L for each star, and make the proper reductions, this formula will become‡

$$\begin{array}{rcl} \mathrm{V} \times (\cdot 00273\,\alpha - & \cdot 00213) + & \\ \text{for January } 1 = & \cdot 00000\ \mathrm{V} & \\ 11 = & \cdot 00273\ \mathrm{V} & \times 10 \\ 21 = & \cdot 00273\ \mathrm{V} & \times 20 \\ 31 = & \cdot 00273\ \mathrm{V} & \times 30 \\ \text{February } 10 = & \cdot 00273\ \mathrm{V} & \times 40 \\ \&\text{c.} & \&\text{c.} & \&\text{c.} \end{array}$$

α being, as before, the right ascension of the star, converted into the decimal part of 24^h. These values being added to

* As an example, take the case of α *Aquilæ* on the *tabular* April 11th. The *true* longitude of the sun on that day, at the time of the culmination of the star, will be 19° 20′ 6″ + 1° 53′ 27″ = 21° 13′ 33″. Consequently by Mr. Herschel's tables the amount of the aberration in right ascension will be $-0^s\cdot 0600$; and of the solar-nutation $-0^s\cdot 0536$.

† Mr. Herschel's Table I. gives the annual vari tions for *mean solar* days, and not for *sidereal* days.

‡ As an example, take the case of α *Aquilæ*, whose annual variation (in right ascension) is equal to $2^s\cdot 924$; the proportional part of which, on January 1, at the time of its culmination, will be $+ 2^s\cdot 924$ ($\cdot 00224 - \cdot 00213$) $= +0^s\cdot 00032$: which being added to $0^s\cdot 0798$, and its multiples, will give the amount of the annual variation at the time of its culmination on every subsequent tenth *sidereal* day of the year. Thus, on the *tabular* April 11th, it will be $+ 0^s\cdot 7983$.

the

the respective values of the aberration and solar-nutation for each star, on those days, will give the total amount of the correction, depending on those quantities, for each star at the moment of its culmination on every tenth day of the fictitious year, commencing from that moment of time when the sun's mean longitude at mean noon at Greenwich is assumed as exactly 281°.

9. Having thus deduced the sum of the values of annual variation, aberration and solar-nutation for each star, and for every tenth day of the fictitious year, from January 1 to December 37 *; they must be arranged into tables, with their differences annexed, and are then ready for use. It is in this manner that M. Bessel has computed and arranged his subsidiary tables for the corrections (in right ascension) of 36 Greenwich stars, inserted in the *Konigsberg Observations* for 1818; and which may be found in Dr. Pearson's valuable Astronomical Tables, pages 149—152. And it is from those subsidiary tables that M. Schumacher annually computes and publishes his apparent places of the Greenwich stars. It is in this manner also that M. Bessel has computed and arranged the Tables in his *Fundamenta Astronomiæ*, page 72, for determining the corrections (in right ascension) of 14 principal Greenwich stars, for the reduction of Bradley's observations.

10. All these tabular values are computed, as I have already observed, for a *fictitious* year (of $366\frac{1}{4}$ *sidereal* days) which commences from an assumed epoch, depending on a given mean longitude of the sun. But, since the mean longitude of the sun at mean noon at Greenwich on January 1, 1800, is not exactly 281°; and since it is never the same at the commencement of each civil year, a correction is required for reducing the values, in the proposed tables, to the true epoch, according to the civil mode of reckoning time. And a further correction is likewise required for the *longitude* of the place of observation; on the presumption that the proposed tables may be used on a different meridian from that of Greenwich. These two corrections are precisely similar to those which I have pointed out in the Introduction to the *New Tables for facilitating the computation of Precession, Aberration and Nutation*, alluded to at the commencement of this communication: and therefore it will be unnecessary to dwell further upon them in this place.

* The year is continued to the fictitious date of December 37, in order to complete the decades, and thus facilitate the computation of the differences. For a similar reason the computation of the lunar-nutation is extended to December 67.

11. There

11. There is, however, another correction necessary in the application of these tables, to which I have not yet alluded. It is well known to all practical astronomers that every star will once in every year culminate *twice* in a mean solar day, when the sun has the same right ascension as the star: and the fictitious day will then have *gained a day* on the civil mode of reckoning astronomical time. This correction is common to all the stars: and when the annual values are computed it is usual to annex an asterisk to the interval which includes the day above alluded to; and the intervals, so marked, will comprehend *eleven* culminations of the star. For those stars also whose right ascension is between 0^h and $18^h\ 44^m$ we must make a *further* addition of unity to the given date from the commencement of the year to the day on which it is in conjunction with the sun. These corrections M. Bessel denotes by the letter i: so that the Argument for entering the tables will be

$$T = \tau + i - x - l,$$

where T denotes the given day, according to the civil mode of reckoning astronomical time, from noon to noon: τ the same nominal date in the tables; i a number which must be taken equal to 0, 1 or 2, according to the circumstances of the case*; and x and l the same as in my Introduction to the *New Tables of Precession, Aberration,* &c. already alluded to.

12. These three corrections,—viz. 1° for the commencement of the year; 2° for the day of culmination with the sun; and 3° for the longitude,—are all that are required in the use of these tables. The argument being once found for the given year, the requisite differences for the computation of the *annual* tables are easily deduced, in most cases by inspection, and always very readily by the assistance of a small auxiliary table of proportional parts.

13. The *lunar-nutation* may be computed for intervals of 100 days only: for, the motion of the moon's node is so slow that it will be unnecessary to compute for any smaller intervals. The mean longitude of the moon's node on January 1, 1800, when the mean longitude of the sun was 281°, was, by the recent tables of M. Damoiseau, equal to 33°·2107: and the

* If the right ascension of the star is *greater* than $18^h\ 44^m$, i is equal to 0, from January 1 to the day on which the sun's right ascension is the same as that of the star; and, after that period, it is equal to 1, to the end of the year. If the right ascension of the star is *less* than $18^h\ 44^m$, i is equal to 1 from January 1 to the day on which the sun's true right ascension is the same as that of the star; and, after that period, it is equal to 2, to the end of the year. Thus, for *α Aquilæ*, April 10th, according to the civil mode of reckoning astronomical time, will be equal to the *tabular* April 11th, because in this case i is equal to 1.

mean

mean motion of the moon's node in a tropical revolution of the sun being $-19°{\cdot}3417$, we may obtain, by simple addition, the mean place of the moon's node on January 1, of any subsequent fictitious year, commencing when the sun's mean longitude at mean noon is 281°. The mean motion of the nodes, in 100 *sidereal* days, is $-5°{\cdot}281$. But these days should (for each star) be computed from the moment of time when the sun's mean longitude is equal to

$$280°\ 13'\ 57'',88 + \alpha \times 58'\ 58'',6417$$
$$= \text{Jan}^{y}\ 0^{d}{\cdot}21944 + \alpha$$

α denoting, as before, the right ascension of the given star expressed in the fractional part of a day. If therefore ☊ denote the mean place of the moon's node, for any given star, computed for the epoch January $0^{d}{\cdot}21944 + \alpha$, we shall have the mean places of the moon's node, for the respective periods as under*: viz.

for Jan.	1	= ☊		
April	11	= ☊	−	5°·281
July	20	= ☊	−	10 ·562
Oct.	28	= ☊	−	15 ·843
Dec.	67	= ☊	−	21 ·124

The year in every case being supposed to commence when the mean longitude of the sun at mean noon at Greenwich, on January 1, 1800, is presumed to be exactly 281°. But, it will be found that great accuracy in this respect is not essentially necessary, when it concerns only the lunar-nutation.

14. The mean place of the moon's node being computed for those periods in any given year, we may readily deduce the place of the node for the same days in any following year, by merely adding $-19°{\cdot}3417$ to each of such values: this being (as I have already observed) the motion of the moon's node in a tropical revolution of the sun.

15. Having thus determined the mean longitude of the moon's node for every *hundredth* day of the year, we must enter Mr. Herschel's tables with the Arguments

$$(☊ + N') \text{ and } (2\,☊ + N^{iv})$$

and having deduced the *lunar-nutation* for those days, we may

* As an example, take the case of α *Aquilæ*, whose right ascension, reduced to the fractional part of 24 hours, has been already deduced equal to ·82118; consequently we must compute the mean place of the moon's node for January 0·21944 + ·82118 = January 1·04062. The position of the moon's node for that moment of time is 172°·955; which being added to −5°·281, and its multiples, will give the position of the moon's node, at the time of the culmination of the star, on every subsequent hundredth *sidereal* day of the year. Thus, on the *tabular* April 11th, it will be

readily

readily determine the amount for every *tenth* day, when we wish to apply it to the computation of the *annual* tables *. The values of N' and N^{iv} are given by Mr. Herschel. M. Bessel has, in the formation of his subsidiary tables, added the *mean place* of the star, at the commencement of the year, to the amount of the *lunar-nutation:* by which means he saves the computer the trouble of one addition. But, with deference to so great an authority, I would suggest the propriety of keeping those quantities distinct.

16. Having thus given a sketch of the manner in which auxiliary tables may be formed, so as to render Mr. Herschel's tables more generally applicable and useful, and at the same time to enable us to obtain more easily the necessary corrections for the Greenwich stars, I trust that some one will be induced to pursue the subject still further, and endeavour to procure the actual computation of such auxiliary tables for the correction of *all* the principal stars now observed at the Observatory at Greenwich. The number of those stars was formerly 36; and these are the stars whose corrections in *right ascension* have been tabulated by M. Bessel: the corrections in *declination* being still a desideratum. The number in Mr. Herschel's tables is 46: and the whole of these might be tabulated in the manner here proposed, without any considerable trouble. But Mr. Pond has recently extended his list to 60 stars, whose apparent places are now given annually in the Nautical Almanac: and at the end of the volume for 1829, is given a Catalogue of *one hundred* principal fixed stars. Whether it is intended to give the apparent places of the whole of these, I know not; but it is evident that the more the list is extended, the more desirable it will be to save the time and labour of the computer: and in no way can this be so effectually done as by the tabular arrangement here proposed.

As your Journal appears to be very extensively circulated on the continent, as well as in this country, I beg leave to take this opportunity of correcting a slight typographical error in my Introduction to the *New Tables for facilitating the computation of Precession, Aberration and Nutation*, which may probably mislead some persons who may employ those tables for a *different meridian*, to that of Greenwich. The error occurs in page xx, where the *accent* has been placed on the wrong *h*: therefore in line 10 for (h) read (h'), and in line

* As an example, take the case of *α Aquilæ* on the *tabular* April 11th. These values will be found equal to $-0^s{\cdot}2065 + 0^s{\cdot}0039 = -0^s{\cdot}2026$.

12, for

12, for h' read h. The subsequent *formula*, in line 14, will then be accurate: but the *cases* mentioned in page xxv require a slight correction, and should be as follow:

Case 1. Arg. = Feb. 10 + (·500 − ·378) = Feb. 10·122
Case 2. Arg. = Feb. 10 + (·750 − ·378) = Feb. 10·372
Case 3. Arg. = Feb. 10 + (·250 − ·378) = Feb. 9·872
Case 4. Arg. = Feb. 10 + (·125 − ·378) + ·018 = Feb. 9·765

This error has likewise led to the inaccurate expression $x - l$ in pages xxii line 18, xxiii line 9, xxiv lines 1 and 5, and xxv line 10; in each of which places it ought to be $x + l$.

It is evident that this error will not affect the argument of the Tables, when they are used in *this country*, or at any of the observatories in the neighbouring states. But, as it might probably mislead a computer under a more *distant meridian*, unless previously detected, I have taken the earliest opportunity of making the error known; although it is manifest that the effect will seldom be of much importance.

Jan. 23, 1827. FRANCIS BAILY.

XXI. *Investigation of the Heat extricated from Air when it undergoes a given Condensation.* By J. IVORY, *Esq. M.A. F.R.S.**

CONCEIVE a quantity of air confined in a close vessel, and let heat be applied to it, the pressure remaining invariable, till it is expanded to a given volume. Again, taking the same mass of air in its first state, let the dimensions of the vessel be suddenly enlarged till the air has acquired the same volume to which it was before expanded by heat: the air within the vessel will become colder, and after a short moment of time will resume its first temperature. We must therefore infer that air, when its volume is increased, absorbs heat, which occasions the coldness; and that the coldness disappears because the loss of temperature is supplied by the communication of heat from the surrounding bodies. That this is a true account of the matter, and that no heat is lost, it is easy to prove; for if the vessel containing the expanded air be reduced to its original bulk, the heat before absorbed will be extricated as the air contracts, producing a rise of temperature which is soon dissipated. Now let heat be applied to the expanded air, while its volume is kept from changing, till the temperature is raised to the same degree as in the first operation: it it evident that the air will now be in

* Communicated by the Author.

the same condition to which it was before brought by the agency of heat alone. For, in both cases, there is the same volume and the same temperature, and consequently there must be the same density and pressure.

And, since the air is precisely in the same state, it must have acquired the same quantity of heat in both processes. It follows, therefore, that when air, under a constant pressure, expands by the agency of heat, the absolute heat which causes a given rise of temperature, or a given dilatation, is resolvable into two distinct parts; of which one is capable of producing the given rise of temperature, when the volume of the air remains constant; and the other enters into the air, and somehow unites with it while it is expanding. Of this latter part there is no perceptible sign, except the cold, or the heat, which appears at the instant of its entrance, or *exit*. The two heats have no mutual dependence on one another, since either of them may be varied in any manner while the other remains unchanged. It is necessary to distinguish them by appropriate names. The first may be called the *heat of temperature;* and the second might very properly be named the *heat of expansion;* but I shall use the well known term, *latent heat*, understanding by it the heat that accumulates in a mass of air when the volume increases, and is again extricated from it when the volume decreases.

We must next inquire according to what law the latent heat accumulates when air expands. When a mass of air, under a constant pressure, varies by the application of heat, I assume it as an acknowledged principle that equal quantities of absolute heat produce equal increments of volume. It is evident that this principle cannot be deduced by reasoning: it must be established by experiment. It is true, so long as an air-thermometer can be reckoned an exact measure of heat; for, if it were not true, the indications of that instrument would be irregular. But, what proof have we that an air-thermometer measures heat exactly? To this it must be answered, that we arrive at the conclusion indirectly, and that there is no direct proof. If we suppose that a given quantity of absolute heat applied to all bodies caused an increment of volume, always the same in the same body although different in different bodies, it is evident that all bodies would indicate by their dilatations the same progression of temperatures. Two thermometers, made of any materials, which agreed in two points of their scales, would always mark the same degrees of heat. Now if we compare two thermometers, one of air and the other of mercury, which have their scales adjusted to the fixt points at which water freezes and boils,

and

and find that their indications agree for a long range of temperature, we must infer that the supposed principle is true in nature for the whole of the interval, and that equal quantities of absolute heat have uniformly caused equal expansions on both scales.

In an air-thermometer, or, which is the same thing, in a mass of air under a constant pressure, the rise of temperature is proportional to the increment of volume. Wherefore, since both the absolute heat and the heat of temperature keep pace with the increase of volume, it follows that their difference, that is, the latent heat, must follow the same law of variation.

And, because it is proved that equal increments of latent heat correspond to equal rises of temperature and to equal increments of volume, we may employ the dilatation of a mass of air to measure the accumulation of latent heat, just as we employ it to measure the increase of temperature. Let v' denote the volume of the fluid, at some fixt temperature, suppose zero of the thermometrical scale; and, the pressure being constant, put v for the volume when the temperature has been raised to τ, and the latent heat i has combined with the air: then, α and β being two constants, it is evident that we shall have,

$$\left.\begin{aligned} v &= v'(1 + \alpha\tau) \\ v &= v'(1 + \beta i) \end{aligned}\right\} \qquad (A)$$

When v' and v are the same in the two formulæ, the two factors $1 + \alpha\tau$ and $1 + \beta i$ are equal: consequently,

$$\beta i = \alpha\tau, \text{ and } \frac{\alpha}{\beta} = \frac{i}{\tau}.$$

The fraction $\frac{\alpha}{\beta}$ is therefore the proportion of the latent heat to the rise of temperature for the same dilatation of the fluid; a proportion which, as has been shown, is constant so long as the air-thermometer continues to be an exact measurer of heat.

The first of the two formulæ necessarily supposes that the air has varied under a constant pressure; but the second is true in whatever manner the volume has changed from v' to v.

Let ϱ' and ϱ denote the respective densities when the volumes are v' and v: then $\frac{\varrho}{\varrho'} = \frac{v'}{v}$ and hence we derive these other expressions, viz.

$$\left.\begin{aligned} \varrho &= \frac{\varrho'}{1 + \alpha\tau} \\ \varrho &= \frac{\varrho'}{1 + \beta i} \end{aligned}\right\} \qquad (B)$$

 Of

Of the two constants α and β, the first is the well-known expansion of elastic fluids for one degree of the thermometer. The fraction $\frac{\alpha}{\beta}$, and consequently β, may be found by ascertaining the heat disengaged from a given mass of air by a given condensation; for the proportion of this heat to the heat of temperature required to produce the same condensation, the pressure remaining constant, would be the fraction sought. I know not that any such experiment has been made with sufficient precision. It appears difficult to perform it with great accuracy, on account of the small quantity of matter in air when compared with the vessels that contain it and with the thermometer, bulk for bulk. But we may employ for the same purpose a very curious and ingenious experiment first made by MM. Clement and Desormes, and afterwards repeated by MM. Gay-Lussac and Welter, which ascertains by the variation of the barometer the heat absorbed or extricated in the changes of volume.

Let p, ϱ, τ denote the barometric pressure, density, and temperature of a mass of air: then

$$p = c\,\varrho\,(1 + \alpha\tau),$$

c being a given number. Put $\varrho' = \varrho\,(1 + \alpha\tau)$; then ϱ' will be the density of the same mass of air cooled down to zero of the thermometer, the pressure being constant; and we shall have,

$$p = c\,\varrho'. \qquad (1)$$

In this formula we consider ϱ' as a fixed density, and estimate all the changes in the condition of the mass of air by means of the variations of the latent heat and the heat of temperature. The air being contained in a close vessel, let a small additional portion of air be forced into the vessel: the consequent condensation will cause an increase of pressure, an evolution of latent heat, and an equal rise of temperature, all which circumstances are easily expressed by proper changes in equation (1), viz.

$$p + \delta p = c\,\varrho' \times \frac{1 + \alpha\,\delta i}{1 - \beta\,\delta i}.$$

After the condensation, the density being fixed, there will be no change in the latent heat; but the heat of temperature will be dissipated in a short moment of time, and the pressure will decrease a little: let p' be the pressure when the condensed air has resumed the general temperature, then,

$$p' = c\,\varrho' \times \frac{1}{1 - \beta\,\delta i}. \qquad (2)$$

A communication must now be opened between the confined air

air and the atmosphere: the condensed air will rush out and expand within the vessel, attended with a decrease of pressure, an absorption of heat and an equal depression of temperature; and the last equation will now assume this form, viz.

$$p' - \delta p' = c\varrho' \times \frac{1 - \alpha \Delta i}{1 - \beta \delta i + \beta \Delta i}.$$

We must here conceive that $\delta p'$ and Δi vary together, and in a very short space of time the pressure will have decreased to its original quantity p: at the instant this is observed to take place, the communication with the external air must be shut, and then we shall have,

$$p = c\varrho' \times \frac{1 - \alpha \Delta i}{1 - \beta \delta i + \beta \Delta i}. \qquad (3)$$

But this state of the air will be momentary only; for the loss of temperature will be supplied, and the pressure will increase a little: let p'' be the pressure when it is observed to be stationary, then finally,

$$p'' = c\rho' \times \frac{1}{1 - \beta \delta i + \beta \Delta i}. \qquad (4)$$

Now by comparing (4) with (3) and (2), we get,

$$\frac{p}{p''} = 1 - \alpha \Delta i,$$

$$\frac{p''}{p'} = 1 + \beta \Delta i;$$

and hence, $$\frac{\alpha}{\beta} = \frac{p'' - p}{p' - p''} \cdot \frac{p'}{p''}.$$

Taking the two experiments, one by MM. Clement and Desormes, and the other by MM. Gay-Lussac and Welter, of which the particulars are given in the *Mécanique Céleste* *, we find $\frac{\alpha}{\beta} = 0{\cdot}354$ from the first, and $\frac{\alpha}{\beta} = 0{\cdot}3724$ from the second. By the two latter philosophers the experiment was repeated in a great variety of circumstances, the pressure being varied from 144^{mm} to 1460^{mm}, and the temperature from -20° to 40° of the centigrade thermometer; and the results were found nearly the same in every case, and upon the whole equal to about 0·3748, or $0{\cdot}375 = \frac{3}{8}$. This experiment was contrived expressly for solving the problem concerning the velocity of sound; for $\sqrt{1 + \frac{\alpha}{\beta}}$ is the factor by which, according to the suggestion of Laplace, the velocity determined by Newton's Theory must be multiplied, in order to get the true velocity. When a method for finding the numerical

* Liv. xii. chap. 3.

value

value of the quantity sought was known, it became a point of great importance to ascertain, by varying the circumstances of the experiment, whether that quantity always retained the same value independently of the different states of the atmosphere; and all the trials that have been made favour the conclusion that it is nearly constant. But the constancy of the factor is now proved *à priori* by the theory here laid down, and is no longer merely an induction from experiments.

Taking $\frac{\alpha}{\beta} = \frac{3}{8}$, we are entitled to enunciate the following proposition, which solves the proposed problem:

The heat extricated from air when it undergoes a given condensation, is equal to $\frac{3}{8}$ of the diminution of temperature required to produce the same condensation, the pressure being constant.

Air, under a constant pressure, diminishes $\frac{1}{480}$th of its volume for every degree of depression on Fahrenheit's scale; and therefore one degree of heat will be extricated from air when it undergoes a condensation equal to $\frac{1}{480} \times \frac{3}{8} = \frac{1}{180}$. If a mass of air were suddenly reduced to half its bulk, the heat evolved would be $\frac{1}{2} \div \frac{1}{180} = 90°$.

Having now solved the proposed problem, I shall reserve what further is important on this subject to a future occasion.

Jan. 8, 1827. J. Ivory.

XXII. *The Bakerian Lecture. On the Relations of Electrical and Chemical Changes. By Sir* Humphry Davy, *Bart. Pres. R.S.*

[Continued from p. 38.]

IV. *On the electrical and chemical effects exhibited by combinations containing single metals and one fluid.*

I KNOW of no class of phænomena more calculated to give just views of the nature of electro-chemical action than those presented by single metals and fluids; and as their results are, with one or two exceptions, entirely new, I shall describe them with some degree of minuteness.—When two pieces of the same polished copper, connected with the platinum wires of the multiplier, were introduced at the same time into the same solution of hydro-sulphuret of potassa, there was no action; but if they were introduced in succession, there was a distinct and often, if the interval of time was considerable, a violent electrical effect—the piece of metal first plunged in being negative, and the other positive.

This result depends upon the circumstance of the production

tion of a new combination, which is negative with respect to the metal; for after the formation of the sulphuret of copper, the plate of copper that has been first plunged into the solution exhibits the same negative state with respect to polished copper, whether introduced into saline solutions, or alkaline or acid menstrua. The electrical effect therefore does not depend on so simple a condition as would at first appear, and it may be in fact referred to the combinations containing two metallic substances and one fluid.

The gray sulphuret of copper is negative, in solutions of hydro-sulphuret, to clean copper, and the superficial coating has apparently similar electrical powers to this substance.

Copper, in the state of protoxide, is negative, not only with respect to metallic copper, but likewise with respect to the sulphuret; a circumstance which explains many singular and apparently anomalous circumstances with respect to the action of hydro-sulphuret on copper. I have often found the order which I have mentioned, of metallic copper being positive with respect to copper that had been a few seconds in solution of hydro-sulphuret, reversed in a singular and capricious way; but on investigating the cause, I found that the copper was tarnished; and on heating any kind of polished copper strongly, so as to produce a thin coating of oxide any where on its surface, it became strongly negative to copper plunged in solution of hydro-sulphuret: the same effect was produced by the action of acids.

There are some singular circumstances connected with the violent and intense chemical action of copper on solutions of hydro-sulphurets, which are worthy of being described. When a piece of copper connected with the multiplier has been for a minute in strong solution of hydro-sulphuret of potassa, on introducing a piece of polished copper connected with the other wire, there is often a violent and momentary negative charge communicated to it, which sends the needle through a whole revolution: it then oscillates, and almost immediately returns, and takes the direction which indicates that the piece first plunged in is negative. This effect continues for some minutes, then becomes weaker; at last the two sides are in equilibrium, and the piece which was first plunged in now becomes positive with respect to the other. The first described of these effects seems to depend upon the discharge, by the clean copper, of the negative electricity accumulated by the contact of the plate first plunged in, before the relative states produced by the metallic contact and the regular currents occur; and the second, to the detaching or peeling off of the coat of sulphuret, which has the effect of exposing

a clean

a clean surface, and which effect is probably occasioned by the oxidation of the positive side of the plate.

There are few electrical actions more intense than those produced by the operation of hydro-sulphurets on copper in these different circumstances; so much so, that I have constructed a Voltaic battery which decomposed water, by six combinations, consisting merely of thin slips of copper, of which one half had been exposed to the solution about a minute before the other half: of course, the oxidating surface was on the side of the clean or latest exposed metal.

With lead, and alloys of tin and lead and iron, there are the same phænomena, but much feebler electrical action, the metallic surface which is first introduced being the negative surface; and the principles of this kind of action are precisely the same as those of copper and hydro-sulphurets.

Zinc, platinum, and metals which have no chemical action on solutions of hydro-sulphurets, produce no phænomena of this kind; silver and palladium, which act powerfully with these menstrua, produce very decided effects; but the compounds they form in them being positive with respect to the pure metals, the phænomena are the reverse of those offered by the more oxidable metals; the surface plunged first into the solution is the positive surface, and it retains this relation in alkaline, acid, and saline solutions, presenting peculiarities dependent upon the change of surface, which I shall refer to again hereafter.

The production of electrical currents by single metals and single fluids, though most distinct in the cases I have just named, yet occurs generally whenever new substances which can adhere to the metals are produced in chemical action. Thus in acid solutions of a certain strength pieces of the same zinc, tin, iron, and copper, exhibit similar phænomena; the surface first plunged into the acid being tarnished, or retaining a slight coat of oxide, is negative to the metal plunged in afterwards, and the relation is sustained in saline or alkaline solutions. The same effect is caused by producing a coat of oxide by heat on the surface, or even by applying it artificially. The oxidated surface is negative with respect to the other.

Zinc, which dissolves in a strong solution of potassa, giving off hydrogen copiously, exhibits exactly the same phænomena in this solution; the tarnished metal, or that first introduced, being negative with respective to the other. Tin likewise in solution of potassa, having been introduced long enough to have tarnished, is strongly negative with respect to polished tin.

Even the noble metals obey the same law. Silver, that has been

been tarnished by the action of nitric acid, is negative to polished silver in diluted acid; and gold and platinum, that have been acted on by aqua regia, are negative in that acid to the clean metals.

The intimate connexion displayed in all these cases between the chemical and electrical phænomena, becomes still more remarkable when the nature of the changes taking place in circles of this kind is considered.

Oxygen, which may be considered as negative with respect to all the metals, and sulphur, which is negative with respect to the oxidable metals, by their combinations with metals respectively positive to them, produce compounds negative with regard to those metals. And in the chemical changes, the results are such as must ultimately restore the equilibrium, hydrogen or sulphuretted hydrogen passing to the negative side, and oxygen to the positive side; so that the oxides are revived; and not only is the equilibrium restored, but the poles sometimes changed. Thus tin that has tarnished in acid, remains for some time negative in solution of alkali, but gradually as the oxide upon it is revived by the hydrogen determined to this surface, it loses its negative power; and the other surface, now tarnished by the action of the alkali, gains this power, whilst the opposite surface becomes positive.

V. *Of electrical combinations, consisting of two imperfect, and one perfect conductor; or two fluids and a metal, or charcoal.*

To understand clearly the nature of the action in this kind of electrical combination, it is necessary to consider the nature of imperfect conducting bodies, water, or saline solutions. These bodies may be regarded as having the same relations to electricities of very low intensity, that elastic fluids have to the electricities of glass, sealing-wax, or the common machine. They communicate the electrical polarities of the metals, but do not appear capable of receiving such polarities, or at least of retaining them; and the electrical equilibrium, when broken in them, seems to be rapidly restored by a new arrangement or attraction of certain of their elements. For instance, if we introduce the positive and negative poles from a very powerful voltaic battery into the extremities of a basin filled with solution of muriate of lime, and place in the circuit different wires of platinum, every wire will possess a positive and negative pole, and there will be no division of the fluid into two parts, one positive, the other negative; and when the two wires are withdrawn, they alone having been used, the electrical appearances immediately cease; and metallic wires unconnected with the battery made to occupy their places, exhibit no electrical phænomena: and in all experiments of this

kind, the well known phænomena of the development of chlorine and oxygen and acid matter at the positive, and hydrogen, alkaline matter, &c. at the negative pole, takes place.

Acid and alkaline matters, when perfectly dry and non-conducting, become on contact negative and positive; as I have shown is the case with oxalic acid and lime; but this effect is similar to that of glass and silk, and the result is a common electricity of tension. And when acids and alkalies combine, their union being apparently the result of the same attractive powers acting on the particles which would produce their electrical relations as masses, they exhibit no phænomena of electro-motion; and such phænomena, when they occur in combinations in which acids and alkalies unite, always depend upon the contact of the metal with the acid and alkaline matter, change of temperature, evaporation, &c. and never on the combination of the acid and alkali.

As a different opinion has been lately started, on high authority*, I shall give the proofs of the truth of this my early view, which appear to me of the strictest demonstrative nature.

A solution of nitre, which is a substance neutral to the contact of noble metals, was introduced into a glass cup containing a plate of platinum connected with the multiplier; pure concentrated nitric acid was placed in another cup, in which there was another plate of platinum joined to the other wire of the multiplier, and the connexion was made by a piece of asbestus wetted in a solution of nitre. At the moment of contact, the needle indicated a strong electrical action, negative on the plate plunged in the acid, and which occasioned a permanent deviation of about 60°.

This arrangement was removed from the multiplier, and another substituted for it, in which strong solution of potassa occupied the place of the nitric acid, being in contact with platinum in one cup, and solution of nitre in the other, with the same communications. The deviation was now much weaker, about 10 degrees, and the platinum in the solution of potassa was positive.

The nitric acid and the solution of potassa were now connected in the arrangement by a piece of clean asbestus, moistened in a concentrated solution of nitre; the deviation of the needle was to about 65°. In this instance there was no chemical action of the fluids on each other; for they had no tendency to mix rapidly with the solution of nitre, which being of less specific gravity than either of the other solutions, remained in the asbestus; and there was no effect beyond that of the metallic contact of the platinum with acid and alkali.

* That of M. Becquerel.

A piece

A piece of asbestus, of nearly the same size with the other, but dry, was now substituted for the moist asbestus, so that the acid and alkali combined by capillary attraction producing heat: at first, the deviation was rather less than in the former instance; but as soon as the combination was complete, the needle stood exactly at the same point, proving that no electricity was developed by the combination, any more than by the indirect communication of the acid and the alkali.

After trying the effects of the contact of fluid acid upon platinum by the arrangement with solution of nitre, and finding that oxalic acid was the acid among the powerful ones which produced the slightest deviation of the needle, or the smallest negative effect, I employed this acid and solution of potassa, exactly in the same manner as the nitric acid in the experiment just detailed; as the joint action of the acid and alkali on the platinum was only to produce a deviation of 7 or 8 degrees, it might be suspected that any electrical action produced by combination might be more easily manifested; but no such effect occurred; and whether the communication was made by combination through dry asbestus, or through asbestus wetted in a saline solution, the effect was precisely the same.

Again,—the two surfaces of platinum were placed in contact with strong solutions of nitre, and the communication made between them by solution of potassa and nitric acid; there was no electrical action, though the chemical combination was intense. But when the fluids were mixed, so that a little acid touched one plate of platinum and a little alkali the other, electro-motion immediately began: and in using muriatic acid and solution of ammonia, which, being lighter than the saline solutions, very soon came in contact with the platinum, the effect commenced almost immediately, and continued for some time to increase.

Again,—I placed pieces of paper coloured with litmus and turmeric, and moistened in solutions of nitre, upon two surfaces of platinum connected with the multiplier; they were covered with a stratum of porcelain clay wetted with the same solution, a stratum of clay moistened with muriatic acid was placed above on one plate, and a stratum moistened with solution of ammonia above on the other, so as to make a contact in which there should be action upon a large surface without direct communication with the metals. In several experiments of this kind there was no electro-motion; and whenever it was perceived, it was found that either the acid, or the alkali, or both, had penetrated through the clay, and touched the metals so as to change considerably the colour of

the papers, which were placed as indications of the correctness of the experiment.

Having brought forward what appear to me decided proofs n this subject, I shall now proceed to investigate the operation of the metals and fluids in combinations containing two of the latter substances. At first I was surprised to find that platinum acted so powerfully with nitric acid, which undergoes no chemical change by contact with it, and suspecting that it might arise from the presence of minute portions of muriatic acid or muriatic salts, I took great pains to exclude these substances by washing the platinum in distilled water, not touching it with the hands, &c. but when the conditions were those of perfectly clean and pure platinum and perfectly pure nitric acid, the phænomena were the same. Similar reasonings may be applied to solutions of potassa, soda, &c. which do not chemically alter platinum by contact, and yet render it positively electrical with respect to platinum in water or saline solutions. It must however be called to mind that the oxygen in nitric acid, and the metals in the alkalies, have attractions of a very decided kind for platinum; and in taking the scale of electro-negative bodies, solutions of chlorine, or nitro-muriatic acid, produce a more powerful electrical effect on platinum than nitric acid, nitric acid than muriatic, and muriatic than sulphuric.

When platinum is brought in contact with an acid, the pole touching the acid is negative, the opposite pole is positive, as I have found by the condensing electrometer; and the reverse is the case when it touches an alkali; so that the circulation of the electricity is from the metal to the alkali, and from the acid to the metal.

Rhodium, iridium, and gold, act in combinations consisting of acid and alkali, on which they have no chemical effect, exactly like platinum; the surface of the metal in the solution of alkali being positive, that in the solution of the acid, negative. With silver and palladium the electricity is greater, particularly if nitric acid is used; and with charcoal and oxidable metals, there is the same general result, the action being in general exalted in proportion as the chemical attractions are stronger, provided there are no interfering circumstances: and in combinations of this kind nitro-muriatic acid is more active than nitric, and the order is after, nitric, nitrous sulphuric, phosphoric, vegetable acids, sulphurous, prussic, sulphuretted hydrogen; and, with the alkalies, potassa, soda, baryta, ammonia, and so on.

It is always to be understood that strong or concentrated solutions of acids and alkalies are employed; for in cases where

where the quantity of acid or alkaline matter is very small and the chemical action of the metals strong, there is sometimes a different order. Thus zinc and tin tarnish immediately even in a weak solution of potassa, and, so tarnished, they are negative to the same metals in weak solutions of muriatic or sulphuric acid; but in experiments of this kind it is easy to determine the true circumstances by changing the poles; the negative side, when the energies of the alkali and acid are weak, will be determined by the tarnish or coat of oxide formed.

Solutions of sulphurets act in these combinations like alkali, with circumstances depending upon the formation of new compounds, according to the law explained in the last section. In combinations, of which the elements are hydro-sulphuret and acid, the metal in the hydro-sulphuretted solution is positive, and that in the acid negative; but with alkalies and hydro-sulphurets, and zinc and tin, the metal in the solution of alkali is positive, and that in the solution of hydro-sulphuret, negative: with silver and palladium the opposite order occurs, and with copper there is nearly a balance of powers, or changes of power, dependent upon the circumstances detailed in the last section.

When, in electrical combinations containing one metal, water or a neutro-saline solution is in one of the cups, and alkali or acid in another, the result is usually such as might be anticipated,—the side of the metal in the alkali is positive, that in the acid negative, and that in the neutro-saline solution in the opposite state. There are however certain neutro-saline solutions, which when they contain oxygen or common air, act upon the more oxidable metals, and such have a power or energy of their own; thus zinc, and tin, and copper in solution of common salt, are positive to the same metals in distilled water; and the surfaces of the same metals in weak muriatic acid are positive with respect to the surfaces in water or saline solutions. In combinations, in which weak and strong solutions of acids or of alkalies are the two fluids, both being of the same kind, the electrical action is usually feeble; but the surface in the strongest alkali is most positive, and in the acids the result usually depends upon the nature of the solution; if oxide is formed and deposited, the strongest acid is negative with respect to the diluted one.

The chemical changes produced in combinations of this kind, are best observed in cases where the metals undergo no change; for instance, with platinum, diluted sulphuric acid, and solution of potassa. In this combination, hydrogen soon appears on the platinum in the acid, and a very small quantity of

of gas, which is probably oxygen, on the platinum in contact with the alkali; and that the acid tends to circulate towards the negative surface, and the alkali towards the positive, is shown by the circumstance of the rapid neutralization of the two menstrua, though separated by asbestus moistened in distilled water.

VI. *Of combinations consisting of two conductors of the more perfect class, and one fluid.*

The order in which metallic bodies exhibit electricities on contact, as is well known, is intimately connected with their relative oxidability, the most oxidable metal being positive with respect to all those below it. This law extends likewise to the newly discovered bases of the alkalies and earths. Potassium and sodium, as I have found by bringing them in contact with zinc in a concentrated solution of alkali, are apparently as much positive with respect to this body, as zinc is with respect to platinum and gold.

There is not however any inherent and specific property in each metal which gives it the electrical character; it depends upon its peculiar state—on that form of aggregation which fits it for chemical change. Thus, zinc in amalgamation with mercury is positive with respect to pure zinc, and the amalgam of tin is in the same state with regard to tin; and the metals of the fixed alkalies in amalgam give the highest positive energy to a mass of mercury some thousands of times their weight.

In general, the electricities developed by metallic contact are of a stronger kind than those resulting from the contact of metals with fluids, so that they are not capable of being changed by them. For instance: zinc in acid is positive with respect to all other metals below it in degree of oxidability, though they are placed in alkalies or solutions of sulphurets: there are however exceptions; for instance, with regard to tin, which, when in a strong solution of potassa, is positive to zinc, in an acid solution; and with respect to iron, which, though positive with regard to copper in all acid or neutro-saline fluids, is negative to it in solution of sulphurets or of alkalies. The electro-motion in these instances produced by the contact of the fluids prevailing over that produced by the contact of the metals.

And knowing the energies of the acid and alkaline fluids, it is easy to apply them so as to diminish or enhance the electrical effects developed by metallic contact.

If, for instance, in a combination containing zinc and platinum, we use two fluids, and place the acid in contact with the

the zinc, and the alkali with the platinum, the effect will be exceedingly feeble compared with that produced if the order be reversed, and the zinc be in contact with the alkali, and the platinum with the acid.

The chemical changes taking place in combinations of this kind are always such as tend to restore the equilibrium; the hydrogen and the alkaline body always passing to the negative, and oxygen and the acid to the positive metal.

There is no instance of continued electro-motion except in cases where chemical changes can take place, for even De Luc's or Zamboni's columns do not act when quite dry, and the silver in combinations of this kind, when the negative metal is gold, is uniformly found tarnished: for the exhibition of electricities of tension, however, a very slight chemical action is sufficient, as the quantity of electricity required to give repulsion to light bodies is exceedingly small; but to form electro-magnetic combinations the chemical agents must be of an energetic kind.

As most of the fluids which act powerfully in voltaic combinations contain water, or oxygen and hydrogen, it has been suspected that these principles were essential to the effect: this however does not seem to be the case, for I found zinc and platinum formed powerful electro-motive circles in fused litharge and fused oxy-chlorate of potassa, which are not known to contain water; and I have little doubt that similar effects would be produced by other fused salts containing only acid and alkaline matter.

It may elucidate this part of the subject, which must at best be obscure, to take a view of the changes occurring in one of the simplest voltaic combinations,—that consisting of zinc, platinum, and solution of sulphate of soda. It is a fact that zinc and platinum become electrical by contact, the zinc positive, the platinum negative; and the two kinds of electricity are apparently most intense at the surfaces where they are in contact with the fluid, which is too imperfect a conductor to allow them to neutralize or destroy each other: they consequently exert their attractive and repellent powers upon the elements of the menstruum; acid and oxygen circulate to the surface of zinc, which in consequence is dissolved, and alkali and hydrogen to the surface of platinum, of which the hydrogen is disengaged, and the equilibrium broken by the contact of the metals is restored by the chemical changes; so that a constant circulation, or a current of electricity, takes place, the power of the combination becoming feebler in proportion as the solution is decomposed, and acid accumulated round its positive, and alkali round its negative surface.

In

In cases where acids or acid solutions alone are used, the destruction of one or both surfaces, with the transfer of hydrogen or oxygen, seems to produce the same effect; and the inactivity of single circles or voltaic piles, in which pure water is used, or saline solutions freed from air, seems to show that the destruction of the surface of the oxidable metal is one of the conditions of continued electrical action; and the cessation of the power of De Luc's or Zamboni's piles, is always connected with the tarnish of the imperfect metal employed in them.

Having published many years ago tables of the electro-chemical relations of metals, which have been copied into many elementary books, I think it proper to give them here in a corrected form with some additions, and the differences dependent upon the nature of the menstruum. The metal mentioned first is positive to all those below it in the scale.

With common acids.

Potassium and its amalgams; barium and its amalgams; amalgam of zinc; zinc; amalgam of ammonium (?); cadmium, tin, iron, bismuth, antimony (?), lead, copper, silver, palladium, tellurium, gold, charcoal, platinum, iridium, rhodium.

With alkaline solutions.

The alkaline metals and their amalgams: zinc, tin, lead, copper, iron, silver, palladium, gold, platinum, &c.

With solutions of hydro-sulphurets.

Zinc, tin, copper, iron, bismuth, silver, platinum, palladium, gold, charcoal.

[To be continued.]

XXIII. *A Mode of Heating Water for a Bath.* By Edward Deas Thomson, *Esq.**

CONVINCED of the great utility of the warm bath to health, as well as the comfort of it, I have for some time turned my attention to the best and most œconomical mode of heating a bath; and have endeavoured, as far as possible, to obviate the inconvenience, delay and expense, which are inseparable from the greater number of the methods hitherto in use. The result has been to exceed my sanguine hopes; having obtained a bath containing 40 gallons of water at a temperature of 98° Fahrenheit, in the space of half an hour from the time of lighting the fire. The quantity of coals consumed was *under* 7 pounds, and the whole expense in London, including the

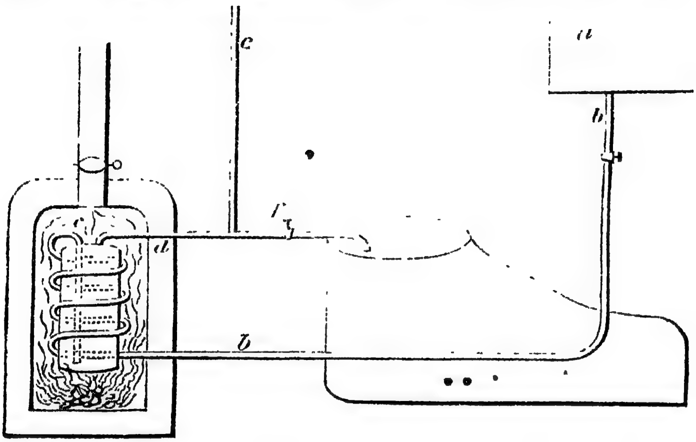
* Communicated by the Author.

faggot,

faggot, did not amount to $2\frac{1}{2}d.$; but as more than usual care was taken in the experiment in question, it may be more fair to estimate the expense on an average at 3*d.* This does not include the wear and tear of the apparatus, which is, however, of a very durable nature.

I shall now proceed to describe the apparatus, and the mode of using it.

A cylinder 18 inches high and 9 inches in diameter, is surrounded by a spiral pipe, as may be seen in the annexed figure:—this pipe communicates with a cistern *a*, which of course must be above the level of the apparatus;—the water passes from the cistern through the pipe *b b* into the cylinder at *c*, and from thence through the pipe *d* into the bath. When

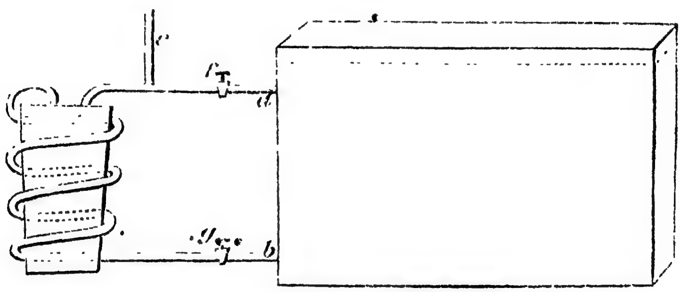


the cock *f* is opened, hot water will flow from the cylinder through the pipe *d* into the bath, and its place will be immediately supplied by cold water from the cistern through the pipe *b b*, thus creating a continual current of water through the boiler, which becomes heated in its passage. The degree of heat may be regulated by partially opening or shutting the cock *f*, by which means the water will flow more rapidly or be longer subjected to the influence of the fire. In case of the water boiling when the cock *f* is shut, the steam will pass off by the *open* pipe *e*, which must of course be raised above the level of the cistern. The pipe being always open provides in the completest manner for the safety of the apparatus.

Various other forms of apparatus and modes of using them have suggested themselves to me; but I have thought it better first to describe the one which had stood the test of actual experiment, and from which so satisfactory a result has been obtained.

obtained. The one in question has been in use for nearly three years, yet, though it was to be apprehended, no symptom of furring has taken place in the pipes. This arises from the formation of the apparatus causing the matter to be deposited in those parts of the cylinder where there is no current. In instances where boiling water or steam is required, and where the liability to fur would be consequently greater, any deposit might be easily drawn off at intervals, by placing a cock at the bottom of the cylinder, which however does not seem to be at all requisite in the apparatus for heating the bath. It may be observed, that the spiral pipe should be at least an inch from the cylinder, so as to allow a complete draught between them.

In instances that admit of the bath being above or on the same level with the apparatus, the following will be found to be a very simple mode, and obviates the inconvenience of attention to the opening and shutting of the cock *f* as above described, the fire being all that it is necessary to attend to



in this instance. The pipes *b* and *d* are made to communicate with the bath, which should be filled, previously to lighting the fire, to a level above the pipe *d*.

From the tendency of heat to preserve an equilibrium, as the water becomes heated in the cylinder it will immediately flow into the bath, and its place is supplied by cold water; thus forming a continual current until the whole is heated to the required temperature, when the cocks *f* and *g* may be shut. In order to prevent any injury to the boiler from the evaporation of the water after the cock *g* is shut; the pipe *b* may be extended to the cistern as in the apparatus first described, and by means of a double way cock at *g*, when the communication with the bath is cut off, it would be opened with the cistern, and *vice versâ*.

In this arrangement the first-mentioned mode of heating the bath might be adopted if desired. In both cases the safety-pipe *e* is, of course, equally requisite.

XXIV. On

XXIV. *On the Finite Extent of the Atmosphere.* By THOMAS GRAHAM, *M.A.**

Edinburgh, Dec. 14, 1826.

TO Dr. Wollaston we owe a satisfactory reason for a limit to the atmosphere, even upon mechanical principles. The idea, that the mere weight of the matter of gaseous substances might afford, at a certain degree of rarefaction, a balancing resistance to further expansion, is certainly beautiful,—a conception worthy of that sagacious philosopher. Mr. Faraday, with his usual felicity in experimental research, has endeavoured to adduce instances of this equilibrium between the expansive power of gaseous matter and its clogging gravity—to give an experimental demonstration of the hypothesis.

Admitting, as we do without hesitation, that the cause assigned would be fully adequate to produce the effect, the question still remains,—but is it really the cause which does produce the effect? The atmosphere may possess some well-known property, which necessarily renders it limited, and the proposal of any supposititious cause may be therefore unnecessary.

Such a property we believe the atmosphere does possess, although we are not aware of its having been noticed with this view previously. The law of the expansion of gaseous bodies by heat and their contraction by cold involves a curious consequence, which has attracted the attention of several philosophers. Bodies cannot exist in that state below a certain temperature. Let us direct our attention to a volume of air at 32° Fahr. It is a well-established law, that for every degree Fahrenheit which the volume of air is heated above that temperature, it increases 1-480th part; and also for every degree which it is cooled below 32° it is reduced 1-480th part of what it was at that temperature. Hence if it should be cooled down 480°, and reduced by so many parts, it would be reduced into a volume infinitely small:—it would really be annihilated. To avoid this absurdity, we are constrained to believe, that all gases would be reduced into the liquid or solid state, by a fall of temperature which does not amount to 480° below the freezing point of water. The proposition, therefore, that the earth's atmosphere cannot exist in the gaseous state at a temperature below $-480° + 32° = -448°$ Fahrenheit, is susceptible of demonstration *ad absurdum*.

Now meteorologists have discovered a law in the atmospheric temperature, which makes this fact available in elucidation of our subject. It has been found that the temperature of the

* Communicated by the Author.

atmosphere decreases as we ascend, and that with considerable regularity. The observations which we possess upon this subject indicate a decrease of 1 degree, for every elevation of about 300 feet. This brings us rapidly to a limit to the height of the atmosphere. Supposing the temperature of the surface of the earth 32°, the air would lose its elastic state at a height which would be less than 480 times 300 feet, or under 27·27 miles. However, without questioning the continuance of this decrease of temperature at great elevations, it is probable that in the higher regions of the atmosphere it is by no means so rapid as in the lower regions, where the law has been verified by observation. For the great source of the heat of the atmosphere is its contact with the surface of the earth, and not in the calorific rays of the sun which it arrests in their progress. Hence the lower strata of the atmosphere will possess a comparatively high and extraordinary temperature, and the fall of temperature as we ascend will appear for some time rapid. But at a certain elevation, the effect of this adventitious supply of heat will be greatly diminished.

The increase of capacity for heat in gases, attendant upon increase of bulk, accounts in a satisfactory manner for reduction of temperature in a mass of air as it is elevated and less compressed. The superior stratum of the atmosphere we may suppose to expand, from its unrestrained elasticity: its temperature is thereby lowered, till at last it arrives at that point which involves the loss of its elastic state. As the liquid state is a physical state of bodies, which implies pressure and a power to maintain the evolution of vapour (certainly in all non-metallic bodies), the cooled and uncompressed superior air will be at once reduced from the gaseous to the solid state. In this way may temperature occasion a limit to the diffusion of the atmosphere.

From the length of time during which the sun's rays continue to be reflected back upon the earth by the superior parts of the atmosphere, after he has sunk beneath the horizon, there is reason to believe that the atmosphere extends in a state of great tenuity to a very considerable height above the surface of the earth, and therefore that the theatre of this condensation is considerably removed. Let us suppose that it is so, and inquire whether its existence would be indicated by any notable effect.

We know well that in ordinary cases, the reduction of a body from the gaseous to the liquid or solid state is attended with a considerable extrication of heat. Light, too, has been observed in condensation following sublimation, particularly in the case of benzoic acid. Now, the superior and condensing

strata

strata of the atmosphere are of a tenuity incomparably greater than that of the vapours whose condensation we generally witness. But this tenuity has been arrived at, at the expense of the previous absorption of much more heat; or in other words, the latent heat of vapours, which is emitted upon their condensation, is in proportion to their tenuity. Hence it is probable, that the condensation of the elastic air into solid particles would be attended with the emission of accumulated stores of light and heat. Would not air, too, it might be asked, emit light upon its complete condensation and loss of physical state, while it may be made to do so by mere mechanical compression? Here, perhaps, we have the cause of that degree of luminosity which is generally associated with the upper regions of the atmosphere, and which has induced Professor Leslie, with that daring originality which frequently characterizes his beautiful speculations, to attribute to them a phosphorescent property.

These luminous appearances will be more frequent and striking at the polar regions, from the temperature, there, approaching more closely to the condensing point of the gaseous substances constituting the atmosphere. Their proper sites will be the thermal poles, or points on the earth's surface of lowest temperature. From late observations, the thermal poles of the earth appear to coincide with its magnetic poles. Let us suppose a determination to condensation to take place in the superior regions of the atmosphere at the thermal pole. The surrounding elastic air would rush in, and expand, to fill the vacuity occasioned by the condensation. But this rarefaction, with its attendant fall in temperature, would frequently be productive of condensation and deposition in these masses of air themselves. In this way, the tendency to condensation, originating perhaps at the thermal pole, would be widely and rapidly propagated; and the attending streams of light would appear to shoot from that point. Here we recognize the brilliant phænomena of the aurora borealis.

It evidently follows from this theory that the atmosphere will be of different altitudes over different parts of the earth, according to their temperature. Within the tropics it will be higher than over the polar regions. Hence the higher parts of the equatorial atmosphere will tend to fall back upon the poles,—a disposition which will coöperate with the inferior current in an opposite direction, to produce a grand circulation of the atmosphere, and to impress a general character upon winds.

XXV. *On*

XXV. *On the Triple Prussiate of Potash.* By R. PHILLIPS, *F.R.S. L. & E. &c.*

FEW substances have more occupied the attention of chemists, than the salt called triple prussiate of potash: but notwithstanding the repeated examinations to which it has been subjected, it will appear on referring even to the latest chemical works, that great difference of opinion still exists, not only as to the mode in which the elements of the salt are combined, but even as to their number, nature, and proportions.

It is not my intention to give a history of the various ideas which have been entertained respecting this substance. I shall first notice the experiments of Mr. Porrett, to whom we are indebted for an opinion now very generally admitted to be true, or at least probable,—that iron, carbon, azote and hydrogen form a peculiar acid, which he has called ferrochyazic acid, and which he considers as the acid of the salt in question.

According to the latest experiments of Mr. Porrett* the triple prussiate of potash is composed of

4 atoms carbon	24	20·168
1 atom azote	14	11·765
1 —— hydrogen	1	·840
1 —— iron †	14	11·765
1 —— potash	48	40·336
2 atoms water	18	15·126
	119	100·

And he considers these elements to be combined as follows:

4 atoms carbon	24
1 atom azote	14
1 —— hydrogen	1
1 —— iron †	14
1 atom ferrochyazic acid	53
1 atom potash	48
2 atoms water	18
1 atom ferrochyazate of potash	119

Berzelius's analysis (*Ann. de Chim. &c.* t. xv. p. 144.) gives;

6 atoms carbon	36	16·902	Cyanogen
3 —— azote	42	19·718	36·620
1 atom iron	28	13·146	
2 atoms potassium	80	37·558	
3 —— water	27	12·676	
	213	100·	

* Annals of Philosophy, vol. xiv. p. 298.

† It will be noticed that Mr. Porrett estimates the weight of an atom of iron at only one half of what it is usually allowed to be.

It

It will be observed that the carbon and azote are equivalent to three atoms of cyanogen; and Berzelius considers the salt as a double cyanide of iron and potassium, containing water of crystallization, or as

1 atom cyanide of iron	26 + 28 =	54
2 atoms cyanide of potassium	52 + 80 =	132
3 — water		27
		213

As the atomic weights of the salt given by these analyses differ so greatly, and the quantities of the elements constituting it do not in any instance agree, it was evidently requisite to repeat the analysis, to a certain extent at least, before any probable theoretical views of its nature could be developed: I therefore made the following experiments. Two hundred grains of the crystallized triple prussiate were dissolved in a mixture of dilute nitric and muriatic acids; the solution was evaporated to dryness, so as to dissipate the carbon and azote, and convert the iron into peroxide. The residuum being dissolved in muriatic acid, and the solution decomposed by ammonia, gave 38·8 grains of peroxide of iron, equivalent to 27·16 of iron or 13·58 per cent.

The solution from which the peroxide of iron had been precipitated, consisting, of course, of muriate of potash and muriate of ammonia, was evaporated to dryness, and the residuum was heated to redness in a platinum crucible, by which the ammoniacal salt was expelled; and there was left chloride of potassium weighing 139·7 grains, equivalent to 73·5 of potassium or 36·75 per cent.

A portion of the triple prussiate reduced to powder was dried in a moderately-hot sand-bath till it ceased to diminish in weight; it lost 12·5 per cent of water.

Assuming, according to the analysis of Berzelius, that the salt is composed of cyanogen and the substances the quantities of which are above stated, it will appear to consist of

Cyanogen	37·17
Iron	13·58
Potassium	36·75
Water	12·50
	100·

These results, it will be seen, agree very nearly with those of Berzelius;—the greatest difference exists between the quantities of potassium, amounting to about 0·8 per cent; while the proportion of iron, which of all the results I obtained comes nearest to any one stated by Mr. Porrett, is more than he has given it by 1·8 per cent.

The

The only question which appears to me to remain undecided, is that of the mode in which the elements that constitute the salt are combined: the simplest view of the subject is undoubtedly that taken by Berzelius, of its being a double cyanide of iron and potassium containing water of crystallization; but he has justly remarked, that the proportions of hydrogen and oxygen are precisely such as would convert the cyanogen into hydrocyanic acid, and the metals into oxides; and according to this view, supposing the water to be formed, and not merely expelled, when the salt is dried, it is a double hydrocyanate, containing no water of crystallization, and composed of

	3 atoms of hydrocyanic acid	81
	1 atom of protoxide of iron	36
	2 atoms of potash	96
		213
Or,	1 atom of hydrocyanate of iron .	63
	2 atoms of hydrocyanate of potash .	150
		213.

It may still further be regarded, according to Mr. Porrett's idea, as consisting of potash combined with a peculiar acid constituted of iron, carbon, azote, and hydrogen. But even admitting these to be the elements of the acid, the proportions must, I think, differ very considerably from those stated by Mr. Porrett.

M. Gay Lussac * considers this acid, as Mr. Porrett does, to consist of metallic iron and hydrogen; but the carbon and azote are in such proportions as form cyanogen, to which Mr. Porrett's analysis is not reducible. According to the former, ferrocyanic acid is composed of

	3 atoms cyanogen	78
	2 —— hydrogen	2
	1 atom iron	28
		108
Or,	6 atoms carbon	36
	3 —— azote	42
	2 —— hydrogen	2
	1 atom iron	28
		108

* *Ann. de Chimie et de Physique,* tom. xxii. p. 322.

According

According to M. Robiquet *, ferrocyanic acid is equivalent to hydrocyanic acid and cyanide of iron, which would give as its composition

	2 atoms cyanogen	52
	1 atom hydrogen	1
	1 —— iron	28
		81
Or,	4 atoms carbon	24
	2 —— azote	28
	1 atom hydrogen	1
	1 —— iron	28
		81

Dr. Ure † is, I believe, the chemist who last paid attention to the composition of ferrocyanic acid: he states the composition to be

Carbon	36·82
Azote	35·29
Iron	27·89
	100·

This analysis differs considerably from all the preceding, not only in the proportion of the elements, but also in the absence of hydrogen: but Dr. Ure allows that he is unable to reduce the results of his experiments to the atomic theory.

Although Berzelius does not admit the existence of such an acid as the ferrocyanic in his paper, contained in the *Annales de Chimie et de Physique*, already quoted; yet in a late work entitled *Chimie du Fer* (p. 181.) he says that ferruginous hydrocyanic acid (*l'acide hydrocyanique ferruginé*) is composed either of 46·57 parts of prussic acid and 53·43 parts of prussiate of protoxide of iron, or of 46·57 of prussic acid, 45·77 of cyanide of iron, and 7·66 of water. Adopting the former of these views, let us examine whether it will not serve to clear up the difficulty which exists not only as to the composition of ferrocyanic acid, but also as to the nature of the triple prussiate of potash.

I consider it as proved by Berzelius that the triple prussiate of potash after it has been moderately heated is in fact a cyanide of iron and potassium; and it must I think in one case, and perhaps in several instances, happen that the metals are converted into oxides: this may be the case, as already noticed by Berzelius, when the salt is in the state of crystals; the

* *Annales de Chimie et de Physique*, tom. xii. p. 294.

† Phil. Trans. 1822. p. 480.

water separated by heating them, on this view, arising from the decomposition of hydrocyanic acid and the metallic oxides, and the union of their hydrogen and oxygen; or supposing the crystallized salt to be a double cyanide containing not merely the elements of water, but water of crystallization, still water may be decomposed when the crystals are dissolved in it, the hydrogen of the decomposed water uniting with the cyanogen to form hydrocyanic acid, and the oxygen with the metals giving rise to potash and protoxide of iron.

Lastly, When cyanide of iron and potassium is dissolved in water, and tartaric acid is added to the solution, water must be decomposed either previously to, or on adding the acid, for bitartrate of potash is precipitated; and if the iron as well as the potassium be also oxidized, we shall have oxygen as well as hydrogen entering into the composition of ferrocyanic acid; and supposing that the three atoms of water expelled from the triple prussiate by heat, do not previously exist as such, but are formed during its action, the salt may be regarded as anhydrous ferrocyanate of potash, consisting of

6 atoms carbon	36	
3 —— azote	42	
3 —— hydrogen	3	
1 atom oxygen	8	
1 —— iron	28	
	117	= 1 atom ferrocyanic acid.
2 atoms potash	96	
	213	

Viewing this as the constitution of the substance in question, it is a diferrocyanate of potash, or composed of one atom of acid and two atoms of base.

I am perfectly aware of the difficulty which attends the supposition that water is formed during the exposure of the crystals of ferrocyanate of potash to heat; but the question is one of probabilities, and Berzelius* has well remarked, "qu'il est impossible de décider si cette eau s'y trouve dans l'etat qui lui est propre, ou si ses principes constituans y étaient employés à la formation du prussiate d'oxidule de fer." I am at present engaged in the prosecution of experiments, by which I hope to throw additional light on this part of the subject, and which, as far as I have proceeded, are confirmatory of the opinions now expressed.

* *Chimie du Fer*, p. 180.

XXVI. *On*

XXVI. *On Capillary Attraction.* *By the Rev.* J. B. Emmett.*

THE phænomena of capillary attraction are amongst the most curious and obscure in nature. The spontaneous rise of liquids between solid surfaces placed very near to each other, proves that corpuscular attraction does extend to a distance equal to several diameters of the particles of the suspended liquid, so as greatly to exceed their weight. Hence the elevation is occasioned by the corpuscular force acting perpendicularly to the axis of the tube, on the same principles as common hydraulic pressure: if the corpuscular force exist, and extend its powers to the distance of several rows of particles of the liquid, all the observed phænomena will result †. Hence also, corpuscular attraction varies reciprocally as the square of the distance from the centre of each particle ‡. Its power

* Communicated by the Author.

† A force acting perpendicularly to the axis of the capillary tube, and exceeding in intensity the force of gravity of the particles at such, still insensible, distances, will act thus. The most remote stratum of the liquid, which is acted upon, tends to the side of the tube, with a certain force; with this force it presses upon all the nearer strata; each of which likewise tends to the tube, with a force which varies according to some function of the distance. Hence each stratum is pressed by the sum of all the tendencies of the strata beyond it, which are sensibly acted upon; the liquid being supposed incompressible. The acting force being inversely as the nth power of the distance from the tube; to a right line, erect perpendicular ordinates, which shall vary in this ratio: draw a curve passing through their extremities, and its area will represent the whole pressure upon the solid: or if solids possess different forces of attraction for the same or different liquids, describe more such curves, making one given ordinate in one to the corresponding ordinate of another curve, as one force is to another force; and their areas will be proportional to these pressures. The liquid cannot be at rest, until this force is balanced by an equal and opposite force: this force can be no other than the weight of the elevated column: and that such a column will be raised, is evident from the principles of hydraulic pressure. The hypothesis of the attraction of an annulus of the tube raising the liquid cannot explain the phænomena; and particularly that of the rise of the liquid around a rod partly immersed: for no annulus can be found, which has not an equal and equidistant annulus, exerting an equal force in an opposite direction.

‡ For (*Newtoni Princip.* lib. i. prop. 87.) supposing a force of attraction to vary reciprocally as the cube of the distance; if similar solids be taken, of equally attracting matter, they will equally attract corpuscles, similarly situated. Now from the specific gravities of a liquid and of the same matter in a solid state, the ratio between the diameter and distance of adjacent particles in the liquid may be known. Now, the force of attraction between some solids, as glass, and some liquids, as water, so greatly exceeds the weight of the liquid particles, at a distance equal to several of their diameters, as not only to support themselves, but an indefinitely great number besides them. Form an elementary cone, whose vertex shall be in the axis of the capillary tube, and whose axis shall be perpendicular to the axis of the

power is astonishingly great *; indeed, since it increases when the aperture is diminished, no limit of its force can be assigned. The laws which it obeys are imperfectly known: whilst mercury is depressed by the immersion of glass, wood, and perhaps all non-metallic bodies; it rises about a surface of gold, silver, lead, tin, and most other metals, provided the surface be clean; the thinnest film of oxide prevents the effect. If a tube of glass be used, whilst water is elevated to a considerable altitude, alcohol, which is lighter, is much less raised; and mercury, the heaviest known liquid, is depressed.

The following are some of the results obtained:—Tube 1st. Water was elevated 4 inches, 5·75 tenths; solution of subcarbonate of potash, nearly saturated, 4 inches, 4·5 tenths; muriatic acid (concentrated) 3, 3·5; solution of loaf-sugar (1 sugar, 4 water) 3, 2·5; alcohol diluted with 10 parts of water 3, 2; sherry 2, 4; spirit 25 per cent under proof 1, 9·5; alcohol 1, 9·5 †. Tube 2d. Water 4 tenths of an inch; nitric acid 3 tenths; refined whale oil 1·5 tenths; oil of lavender 1·5 tenths. So far as I can draw any conclusions from the experiments which I have made, when glass is the solid made

the tube: divide this by planes perpendicular to its axis; making the thickness of each slice proportional to the distance of its nearest surface from the vertex; and a particle in the vertex will be equally attracted by each slice. Hence if two tubes be taken, having different apertures, the glass in each being proportional in thickness to its aperture, the water must be equally elevated by each tube. Or form a sphere of glass, and at several diameters place a drop of water: it will gravitate more to the sphere than to the earth. Now since no such effect takes place, and since tubes of equal apertures elevate equal columns, whatever be their thickness; and since the most minute film of oil within the tube prevents its action; the force is that of the surface only, or of particles at a distance below it, which is less than any measurable distance; and the particles are not endowed with a centripetal force varying inversely as the cube of the distance. If any other law of force, as 4th, 5th, &c. be assumed, its action may be investigated in a similar manner, by the same proposition; which proves that such a force cannot be purely corpuscular, as those commonly called corpuscular really are; that its effects on capillary attraction will be such as are here named; and besides, that its effects on the aphelia of the planetary orbits must be very sensible. Since then none of the effects of a force varying according to any power of the distance, but the square inversely; we conclude that matter possesses no power of attraction, but that developed by Newton.

* The method of cutting large pieces of stone from the quarry, for oil-mill-stones, which consist of a circle 7, 8 or 10 feet in diameter, and 1½ thick; that of elevating immense weights by moistening a rope well stretched, may be quoted as notable examples.

† A capillary tube cannot be made to answer the purpose of the hydrometer: for if a very minute quantity of alcohol be added to water, it is depressed to nearly the level of spirit itself: and when spirit approaches nearly to the strength of the Excise proof, a very considerable difference in its strength produces little effect in the height of the column.

use

use of, inflammable liquids, *i. e.* those which tend to the negative pole, are least elevated: however, I dare not yet assert this as a fact. I have been some time engaged in a series of experiments on this subject, which have led to very curious results, and which I am continuing at present: they will be regularly communicated to the Philosophical Magazine ; and will, I hope, develope the laws obeyed by this force. A full examination of its phænomena will greatly tend to elucidate those of chemical action.

It is stated, I believe, in all books of philosophy, that the altitude of the column raised by the power of capillary attraction is not affected by changes of temperature. I find that it is depressed by heat and elevated by cold. If a capillary tube be immersed in boiling water, the column cools as it rises, and consequently no effect can be expected. I therefore employ the following method:

The capillary tube (about $\frac{16}{1000}$ to $\frac{30}{1000}$ of an inch in internal diameter) is placed in a test-tube, containing the liquid: the tube is filled; and the fluid allowed to fall, till it becomes stationary. When water is used, it is pure; it is also boiled, to expel the air it contains, immediately before the experiment is made. The following are some of the results:

	Inches.	Tenths.	
Altitude of the column of cold water	2	4·5	
Ditto boiling	2	0·5	mean of 3 exp.
Depression caused by heating the water		4·0	
Proof-spirit, cold	0	9·5	
Ditto, boiling	0	8·75	1st exp.
Ditto, ditto	0	8·75	2d ditto
Depression by heating.		·75	
Altitude of column of water at 70°	2	1	
Altitude by immersion in snow . . .		2·5	rise 1·5 tenth.
Altitude when boiling	1	8	
Difference between the extremes		4·5	
Weak sulphuric acid, which had been kept in a badly stopped bottle for three years,—cold .	2	0·5	
Heated nearly to ebullition .	1	9	∴ maximum depression $\frac{2\cdot5}{10}$.
Cooled till it was rather warm	2	0	
Boiling rapidly	1	8	
Quite cold	2	0·5	

The

The quantity of the depression in the above experiments is not to be regarded as rigidly accurate, since the apparatus employed is imperfect: the future experiments (of which you will receive an account in time for your next Number,) will be made with an apparatus in which the index will be moved by means of a fine micrometer-screw: the above, however, prove unquestionably that heat depresses water, and some, probably all, other liquids.*

It would be premature to assign the cause to which these phænomena are to be ascribed: yet since the diminution produced in the density of the liquid by heat, cannot give rise to the effect †, it appears highly probable that the repulsive force of caloric, acting between the solid and the particles of the liquid ‡, being augmented by an increase of temperature, the sensible force of attraction, *i. e.* the excess of the attraction above the force of calorific repulsion is consequently diminished, and therefore the height of the suspended column is reduced.

Should this be the case, the phænomena of capillary attraction will afford a ready and accurate means of ascertaining the relative intensity of the attraction of various bodies, and the ratio between the force of attraction, and that of repulsion at different temperatures, together with many other departments of chemical science.

[To be continued.]

* If the conjectured be the true cause of the phænomenon, mercury also will be depressed by heating the tube.

† For, let A be the density of the liquid, when cold; a, that when heated; H the altitude of the cold, and h that of the hot column. A H will be the pressure upon a given area, when cold; and $a\,h$, that when hot. The attraction of the glass is equal to this pressure: this attraction is proportional to the density of the liquid (the liquid being the same in different states of density); *i. e.* in the cold, to A, and in the hot to a; or $AH : ah :: A : a$; therefore whilst the density of the liquid is changed by the application of heat, the altitude of the suspended column remains constant.

‡ That the particles both of liquids and gases attract those of solids, and that the force of repulsion of caloric acts mutually between them, may be proved in several ways. Oxides of manganese, iron, lead, silver, mercury, and many other metals, are either wholly or in part reduced by the application of heat; so are most carbonates, some muriates, all nitrates. Now the fact of their combination proves that the particles of the solid attract those of the gas; and that of the decomposition, that they mutually repel each other by reason of their calorific atmospheres; in like manner water combined with subcarbonate of soda, sulphate of soda, borax, and many similar bodies, even being equal in weight, in some cases, to the dry matter, forms with them dry solid crystals: hence it is retained by a powerful force of attraction: the application of heat first fuses the crystals, then evaporates the water: hence the repulsive force of caloric is mutual between the particles of a solid salt and the water of crystallization.

XXVII. *A new*

XXVII. *A new Method of bleaching and preparing Flax. By the Rev.* J. B. Emmett.*

ON account of the great distress which prevails in most of the manufacturing districts, I have been induced to present to the public the following means of bleaching and preparing flax and tow, by a simple, easy and cheap process, whereby it is reduced to a beautiful degree of whiteness, becomes possessed of a silky lustre, and is made sufficiently fine to be manufactured into the finest goods; hoping that it may become the means, in the hands of opulent manufacturers, of giving employment to some of the workmen, who are unable to meet with it.

The process is as follows: Steep or boil the flax or tow in a weak solution of subcarbonate of potash or soda, in order to extract the colouring matter, resin, &c. I prefer the subcarbonate to the pure or caustic alkali, because, however diluted the latter may be, its powers of corrosion are so great that if it extracts the extraneous matter perfectly, it will almost certainly diminish he strength of the fibre; whilst I find that it may be thoroughly extracted by the former, without producing any such effect: this I have proved by experiments made upon rather large quantities. Wash it thoroughly from the alkali.

The bleaching-liquor is prepared in the following manner: Reduce perfectly fresh burnt charcoal of soft porous wood, as willow, or fir, to a very fine powder; tie up the powder in a bag made of cloth of a close texture; immerse it into cold soft-water, and work it by pressing it with the hands, until such a quantity shall be diffused through the water, that on rinsing a little flax through it for a few minutes, and then withdrawing it, it shall be lightly blackened. Put into it the flax to be bleached, taking care that each parcel shall imbibe it to its middle. When all is put into the liquid, the water, on being well agitated, ought to be clouded by the charcoal. I cannot specify the exact proportion, as I observed it no further than this,—that I always used more than was actually requisite: in bleaching 6 or 7 pounds, I never used more than half an ounce. Agitate the liquid, and press the flax under it several times in the day, in order to bring as much charcoal as possible into contact with it. After about 20 or 24 hours, remove it from the liquid, having it well wrung; put it into a second which may contain less charcoal: agitate as before, and after the same interval of time, examine a small parcel by washing

* Communicated by the Author.

it with soap and hot water: if the colour be good, remove it from the charcoal-liquid; if not, allow it to remain another day or until it becomes white: 2 or 3 days are amply sufficient if the process be well conducted. It is advantageous to spread it out thinly upon the grass, wet as it is, and having the charcoal in it, taking care to turn it frequently for a few days: the charcoal greatly disappears, and the surface acquires a pearly appearance.

The flax is now to be rinsed in a large quantity of water: then to be washed thoroughly with soap in hot water, till it is quite clean; the soap must then be washed out by cold water, and the flax dried; if on the grass, exposed to the sun and air, the better.

Before washing out the charcoal with soap, the lustre of the fibre will be improved by steeping it for 8 or 10 hours in water just soured with sulphuric acid; if this process be continued too long, the fibre will be weakened. The acid-steeping is not essential, except the flax be intended for some particular uses.

The charcoal is easily washed out, and that perfectly, with soap. The ultimate fibres are perfectly separated: they are so much finer than silk, that I use them in the quadrant, transit and micrometers: the lustre is precisely that of silk; the strength of the fibre is not at all impaired. It takes such colours as I have tried—blue, pink and yellow—perfectly. The finest thread may be spun.

Having made public the process, and particularly on account of my reason for so doing, I hope that manufacturers and others who can forward the introduction of the material, will bestow some attention upon the subject.

Any persons shall be provided with samples perfectly prepared, by addressing me (post-paid) at Great Ouseburn, near Boroughbridge, Yorkshire.

P.S. It may probably be worthy the attention of the Irish; and particularly since the process may be performed by individuals at their own houses, and may give employment to many paupers in the work-houses.

XXVIII. *Description of New Succulent Plants.* By A. H. Haworth, *Esq. F.L.S. &c.*

OF the new Succulent Plants described in this paper, one half were sent to the royal gardens of Kew, from South Africa, by Mr. Bowie; and one of these latter plants has proved to be a new species of *Bowïea*, whose flowers, as Mr. B. assures us, are always in umbels, in the places of their natural occurrence.

occurrence. But for such assurance, we might have thought the plant which very recently flowered at Kew, and which is minutely described below, had not completely developed its inflorescence. This, however, was not the case.

The remaining articles of the Decade have been communicated from other collections and correspondents. One of these, allied to *Tetragonia*, I have thought proper to erect into a new genus; nor will this be wondered at, amongst the almost hundred novelties which I have described in the recent volumes of the Philosophical Magazine.

Chelsea, Nov. 1826. A. H. Haworth.

Decas octava Plantarum Novarum Succulentarum.

Classis et Ordo. Pentandria Digynia.

Genus, Ceropegia *Auctorum.*

stapeliæformis. C. (lurid trailing) ramis prostratis carnosis
1. loreis luridis teretibus subaphyllis simplicibus fusco-marmoratis.

Habitat C. B. S. ubi invenit Dom. Bowie. G. H. ♄.

Florebat in ditissimo regio horto Kewensi Julio, &c. A.D. 1826.

Obs. Habitus *Stapeliarum* (præcipuè *Orbearum* Nob. earumque crassitudine.) *Rami* 3—4-pedales tertio anno, subtùs parcè tuberculatim asperiusculi. *Folia* minutissima ternata remota ferè invisibilia, è locis s. basibus tumentibus persistentibus progredientia, omninò sessilia seu quasi ad caules adnata sine petiolo, subrefracta cordata cuspidata pallida. *Flores* ex alis foliolorum *Stapeliarum* modo, ferè sessiles, at incipientes inapertos vix semunciales solùm vidi.

Classis et Ordo. Hexandria Monogynia.

Bulbine. *Willd. Enum.* 372.—*Nob. in Revis. Pl. Succ.* 32. *Corolla* patens decidua. *Filamenta* barbata. *Sprengel. Syst. Veg.* 2. 7.

bisulcata. B. (double-channelled bulbous) foliis pulposis
2. longè subulatis acuminatis, utraque canaliculatis, radice magno bulboso.

Habitat C. B. S. G. H. ♃.

Florebat in aëre aperto in terrâ prope murum cum aspectu australi, in Novemb. A.D. 1825. Communicavit amicus Dom. R. Sweet, *Horti Britannici, Geraniacearum, Cistinearum, &c. &c.* utilissimus auctor.

Obs. Bulbus magnus secundùm Dom. Sweet. *Folia* (in aëre aperto) pedalia erectiuscula valdè pulposa nec fistulosa, viridia, utraque latissimè, sed internè altiùs sulcata, obsoletéque sulcato-striatula. *Scapus* in nostro exemplo (an semper?) foliis brevior, teres, erectus, calamo tenuior. *Flores* spicati lutei, ut in affinibus: *filamentis* omnibus barbatis.

Obs. B. pugioniformi in magnitudine habituque simillima, certéque in systemate proxima: sed distincta. Distinguitur optimè foliorum sulcis profundis utraque.

Bowiea. *Nob.* in Phil. Mag. Oct. A.D. 1824.

Obs. The discovery of a second species of *Bowiëa* requires the alteration of the generic character, as follows:

Perigonium hexapetaloideum erectum s. patens, cylindricum; laciniis subringenter bilabiatis. *Stamina* inæqualia exserta, inclusave, et cum *stylo* flexuoso declinato-adscendentia.

Herbæ africanæ succulentæ perennes, *foliis, scapis, bracteis*que *Aloïum* propriarum, *floribus* diversis.

myriocantha. B. (umbelled) foliorum marginalibus denticulis
3. numerosissimis; floribus umbellatis.

Habitat C.B.S. ubi invenit Dom. Bowie. G.H. ♃.

Florebat in regio horto Kewensi, Oct. A.D. 1826.

Obs. Caudex senectus incrassato-subconicus, et in nostro exemplo nativo biuncialis; in locis natalibus fortè semisubterraneus.

Folia multifariè effusa vix numerosa subsemipedalia, 4—5 lineas lata, arcuatim patenti-recurva lorato-linearia crassiuscula attenuatim acuminata submucronata, concavo-canaliculata sordidè viridia seu glaucescentia, *subtùs* convexa, rariúsve obsoletè carinulata tuberculato-spinulescentia, asperrima, spinulis minutis respicientibus; *suprà* lævia; ordine sæpè geminato, macularum oblongarum albarum (in medio folii) longitudinaliter dispositarum, rariùs tuberculatim subelevatarum: marginibus (*foliorum*) minutè albo-cartilagineis, denticulis numerosissimis minutissimis albis rectis vel subrespicientibus. *Scapus* vix pedalis erecto-adscendens subflexuosus gracilis teres lævis, infernè nudus, supernè *bracteis* latè adpressis acuminatis plùs minùs membranaceis, et aristatis, superioribus magìs magísque imbricanter approximantibus; supremis supra flores, in capitulo denso conico sterili mortuo membranaceo

branaceo finientibus. *Flores* 6—8 in spuriâ umbellâ erumpentes ex bractearum alis *Aloïum* modo, (nec ut in *Haworthiâ*) *pedunculis* semuncialibus erectis teretibus lutescentè-viridibus. *Perigonium* pedunculo longius, parùm ringens si vidi perfectum, tribus exterioribus laciniis acuminatis crassioribus, harum *suprema* incurva longior erecta infernè sordidè rosea, supernè virescens viridibus nervis, *inferioribus* (laciniis) conniventibus nec patentibus. *Lacinia* tria interiora (perigonii) breviora teneriora incurva (uti priores) sordidè lutescentia carinulâ viridi. *Stamina; filamenta* inæqualia inclusa (in nostro exempl. an semper?) basi perigonii inserta lutescentia; tria ceteris longiora flexuosè declinato-adscendentia, cum *stylo* ab ipso basi flexili. *Stylus* niveus stamina superat, interiores lacinias perigonii æquans, *stigmate* exiguo trilobo luteo. *Antheræ* defloratæ solùm vidi; *polline* aurantio. *Germen* oblongum obtusè sexcostatum.

Obs. I will avail myself of the present opportunity of giving an improved specific character and description of *Bowiëa africana*, as follows: "Foliorum marginalibus denticulis numerosis; floribus spicatis."

Obs.—Folia sublævia. *Flores* patuli, laciniis obsoletè bilabiatis, apice subrevolutis, *genitalibus* exsertis.

HAWORTHIA, *Duval. in Cat. Pl. Succ. in Hort. Alenc.* A.D. 1809.—*et Nob. in Synops. Succ. &c.*

Sectio, CAULESCENTES, rariùs pedales, foliis rigidis 3—5-fariis densè imbricatis, sæpè spiraliter tortis; et sæpiùs saturatè viridibus.

torquata. H. (long, twisted triangular) foliis trifariis sub-
4. patulo-recurvulis sordidè viridibus asperiusculis; caule torquato.

Habitat C. B. S. G. H. ♄.

Floret ut in affinibus.

Communicavit Illustr. Princeps De Salm Dyck, ut var. ejus *Aloe pseudo-tortuosæ.* Sed magìs approximat *Haworthiam pseudo-rigidam*, Salm; foliis quàm in eâ rectioribus, lævioribus pallidioribus. Etiam simulat *H. tortuosam* Nob. at cum foliis minùs rectis pallidioribus tuberculis longè minoribus sineque lente invisibilibus, sed longissimè numerosioribus, inferiorem paginam (foliorum) creberrimè occupantibus.

Classis et Ordo. DODECANDRIA MONOGYNIA.

PHACOSPERMA. *Genus novum.*

Calyx diphyllus.
Corolla 5-petala.
Stamina; filamenta 13.
Capsula 1-locularis polysperma.
Semina lenticularia minuta.

peruviana. P. (Peruvian.) —— *Habitat* in Peru?
5. *Floret* Jun. Jul. &c. G. H. ⊙. s. ♂.

Obs. Herba radice subfusiformi fibroso. *Caulis* pedalis erectiusculus debilis flexuosus obsoletè hexagonus. *Folia* alterna lineari-lanceolata, carinulata subcarnosula viridia internodiis longiora, ad margines hispidiuscula, obsoletè decurrentula, inde caulis angulosus. *Flores* spicati pedunculati mane aperti. *Spicæ* terminales longæ. *Pedunculi* solitarii erecti, bracteâ foliiformi breviores; imi geminati plúsve, filiformes; superiores confertiores sensim breviores subangulati clavati. *Calyx* diphyllus amplus foliolis 4-angularibus rhombeisve erectis crispis, apice carinatis, acumine producto, florem et capsulam amplectentibus. *Petala* 5, obovato-cuneata saturatè purpurea sive rubicunda, basi imbricantia, calyce ferè duplò elatiora. *Stamina* (*filamenta*) brevi- ...ente ramentacea. *Antheræ* (innuptæ) utraque obtusæ aurantiacæ petalis 3—4-plò humiliores. *Stylus* 1, validus brevissimus atropurpureus, staminibus humilior, *stigmate* sexlobulato magno concolore. *Capsula* oblonga subacuta obtusè subtriquetra (rariùs tetraquetra) unilocularis polysperma; *seminibus* minutis lenticularibus nitentibus, è fundo capsulæ pedicellatis. *Semina* vix perfecta, solùm vidi.

Obs. I found this plant in flower in Chelsea garden in June 1825, under the name of *Tetragonia peruviana;* but find no published description of it. It is distinct as a genus from *Tetragonia*, as sufficiently appears above: and other genera will probably recede from *Tetragonia*, as soon as I can procure and re-examine living specimens of the different species in a proper state of fructification.

Classis et Ordo. DODECANDRIA DODECAGYNIA.

SEMPERVIVUM *Auctorum.*

Sectio, GRANDIFOLIA *Nob.* Caules frutescentes succulenti

culenti erecti. *Folia* maxima in rosulas terminalia, cuneato-spatulata, &c.

urbicum. S. (great bicuneate) foliis decurrenter subpetiolatis
6. longissimè cuneiformibus, apicem versùs latissimè obcuneatis, cuspide parvo.

Habitat fortè in Canariis. G. H. ♄.

Flores non vidi.

Obs. Sub hoc nomine occurrit in Hort. Chels. in tepidario: sed in libris nondum inveni. *Suffrutex* nunc subbipedalis, duplò major quàm *S. arboreum;* simplex, *foliis* magìs petiolatis, magísque divergentibus subquadri uncialibus viridibus cartilagineo-ciliatis, apicem versùs minùs spathulatis, cuspide roseo.

retusum. S. (great retuse leaved) simplex: foliis altè cuneatis
7. expansis lævibus ciliatis, apice subcuneatis truncatis retusis.

Habitat in Insulâ Teneriffe, in muris, tectis, &c. copiosè, ubi invenit amicus Dom. Thom. Edwards, succulentarum plantarum cultor; qui in Angliâ benè cultabat A. D. 1824. Præcedenti, simillimum at majus, et satis aliorum retusorum formâ differt. In cæteris convenit, nunc humilius ferè duplò: sed in natalibus locis bipedale magnum potiúsve maximum, secundum Dom. Edwards, foliorum capitibus, latitudine plusquam pedalibus. *Caulibus* ferè semper simplicibus.

Flores non vidi; sed secundum Dom. Edwards lutei et distantiores quàm in *S. arboreo.* G. H. ♄.

frutescens. S. (small tree) simplex: foliis capitatim incurvo-
8. congestis spatulato-cuneatis viridibus ciliatis.

Habitat in Insulâ Teneriffe.

Obs. Cum priore invenit amicus Dom. Thom. Edwards, et cum eo cultabatur A.D. 1824.

S. arboreum, affine, at adhuc solùm semipedale, foliorum capitulis duplò minoribus magìs compactis. In cæteris adhuc quadrat.

Flores non vidi. G. H. ♄.

Classis et Ordo. ICOSANDRIA MONOGYNIA.

CEREUS *Miller.—Nob.* &c.

tenuispinus. C. (long wool-spined) subtriangularis: spinis
9. crebriùs fasciculatis elongatis tenuissimis, lanâ longioribus et ferè laniformibus.

Habitat .... Parvam incipientem plantam solùm nuper

nuper vidi sub hoc nomine inter alias rarissimas plantas in horto Dom. Tate, in vico Sloane-street.

Habitus fortè ut in *C. triquetro* Nob.—*Flores* ignoti: sed prope id locarem. St. ♄.

gracilis. C. (slender, long-spined) suberectus, tereciusculus:
10. spinis antiquis solitariis rectis uncialibus, incipientibus geminatis plúsve, albis.

Habitat in Americâ calidiore. St. ♄.

Obs. Unam plantam 4-entalem virescentem simplicem apud Dom. Loddiges solùm vidi, cum duabus incipientibus ramulis recentèr pullulatis. Plantæ facies est ferè ut in *Euphorbia Hystrix* Auctorum, at adhuc minùs spinosa, duplò brevioribus spinis. An rami in ætate 3-angulares? *Flores* adhuc ignoti. Affinis fortassè *Cerei nani* Kunz: (quod non vidi) et prope id locarem; sed nihilominùs fortè longè major, et sine areolis; ut in *Cereo nano.*

P.S. Having, since my last communication, detected an error in my fourth Decade of New Succulent Plants, page 33, line 13, (in the Philosophical Magazine for August 1823), have the goodness to notice it as follows:

"*For* hæmisphericæ, *read* orbiculari."

XXIX. *On the Accidents incident to Steam Boilers**. *By* John Taylor, *Esq. F.R.S. F.G.S. F.H.S.*

It has been remarked by some practical men who have had most opportunity of examining the circumstances under which the bursting of boilers has taken place, that the causes have sometimes appeared to be not of that simple character which is commonly assigned to them; and that some such accidents have occurred where neither excessive expansive force of steam, neglect of the usual precaution, weakness of material or bad construction, existed to a degree equal to the effect. Mr. Woolf in a conversation upon this subject some time since, expressed to me his opinion of some case where, as he thought, there was ground to suspect the operation of an explosion of gas in the flues, or at least outside the boiler. Any inquiry or discussion into the causes of circumstances which continue to be a reproach to our mode of using steam, must, I conceive, be useful; and my principal object will rather be to provoke it, and to encourage a record of facts, than to propound any particular theory of my own, though I admit that some recent cases appear to countenance Mr. Woolf's idea.

In the mines of Cornwall, and in those of North Wales, the use

* Communicated by the Author.

use of high-pressure steam has become general: in the former district it is, I believe, universal, and is applied to condensing engines not differing very much from Boulton and Watt's construction, among which engines are many of enormous power, and the largest in the world. The steam is commonly so as to balance from 15 to 40 pounds on each inch of the safety-valve; and some difference of opinion exists among the engineers as to the importance of using it at a higher or lower degree of pressure.

It will be necessary to describe the boilers which have been employed, in order to understand the subject, and to notice those which have been subject to accidents; which indeed, as far as I know, have been confined to one sort of boiler,—or at least such accidents as have been attended with any fatal or distressing consequences.

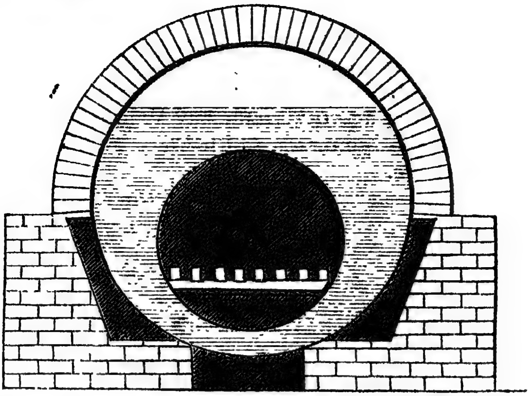
This description of boiler, though appearing therefore to be the most hazardous, is yet most generally adopted; and as it is believed to have some advantages over others in other respects and under certain circumstances, it will probably continue to be generally preferred, or at least until some construction that shall unite these advantages with more perfect security may be brought into use: this, indeed, it will not be very easy to do, as the experiments on boilers have been multiplied to a great extent in Cornwall, and the expense incurred by many of the mines in this way has been so great, that but few of the managers will probably be inclined to enter upon them again without some very clear prospect of success.

The steam boilers which I mean to describe as the most common, are those which are constructed by fixing one tube within another: the interior one containing the fireplace, and the space between it and the exterior containing water, and in the upper part steam. This kind of boiler was, I believe, first introduced by Trevithic for his simple high-pressure engines: he made the outer tube of cast iron, and the inner one, which was often recurved so as to make a double circuit within, of wrought iron. At present both the tubes are made of wrought iron or rolled plates: the form is simply that of one straight tube passing through the other; the ends of the boiler fix the tubes together, so that the interior tube is open at both ends; at one of which is placed the fire-grate, and at the other the smoke and flame pass out, and are conveyed to the stack or chimney most commonly by flues passing under and along the sides of the outer case.

The following sketch will show the cross sections: they are commonly from 20 to 35 feet in length, the diameter of the inner tube from 3 to 4 feet, and of the outer one from 5½

to

to $6\frac{1}{2}$ or 7 feet. The former are usually $\frac{1}{2}$ an inch thick, and the outer case $\frac{5}{8}$ths.



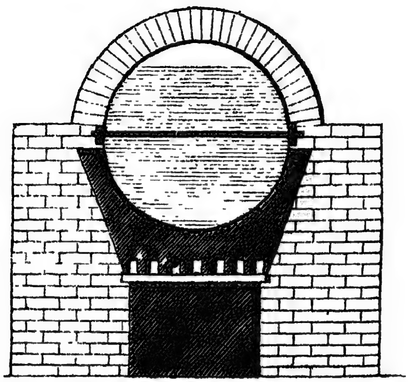
The weakest parts of this construction have generally been supposed to be, the outer tube, by having too great a diameter for the strength of iron used, and the ends of the boiler, which, by being square and riveted to angle iron, are more likely to break than if a spherical form were adopted. It does not appear, however, in practice, that these have been the first parts to give way.

The advantages which this boiler seems to possess over others may be shortly stated. It has been found, by comparing the duty of the engines by means of the monthly reports, and checking this by the observations of the agents, that the fuel goes further in them than in any others yet tried. Circumstances, at first sight apparently trivial, may perhaps conduce to this result. I suspect that the peculiarities of coal of different districts influence more the success of different kinds of boilers than has been generally supposed. In Cornwall all the coal is from South Wales, and is brought from the neighbourhood of Swansea; it is less bituminous than most other coal, is not easy to inflame, but gives a strong and durable fire: it is subject to the objection of producing a great deal of clinker, and this unites with and adheres strongly to any brick-work which the fire may come in contact with, so as to require frequent cleansing of the fireplace. In the boilers I have been describing there is no brick-work near the fire, the clinker does not adhere to the iron sides, and the process of cleansing is easy and rapid; the action of the fire is therefore regular and uninterrupted.

The second kind of boiler used is a single tube made of wrought iron plates of considerable length but of small diameter,

meter, with ends of the same material generally of a hemispherical form; it is placed horizontally, the water occupying by far the larger portion of the space within, and the fire is applied under the bottom part.

The section annexed shows the position of the boiler with respect to the fire: the lengths which have been usually made may be stated at from 20 to 40 feet, and the diameter from 4 to 5 feet. The plates generally employed for making these boilers are ⅜ths thick.



This construction is common, I believe, in America, but they were not much used in England until they were introduced by Messrs. Taylor and Martineau, who have made them in a very excellent manner; and by placing the bar across the centre, as shown in the drawing, and which is repeated at intervals throughout the whole length, they have given them the greatest strength, and rendered them easy to repair. I have never heard of any one of these boilers having burst or caused any disagreeable accident.

In our mines in North Wales I have used them with great advantage, and our agents and engineers prefer them to any other, and find that they generate steam rapidly, and apparently with œconomy; but, as there is no monthly report there as in Cornwall, this point cannot be ascertained precisely.

I expected the same advantage by using them in Cornwall, with the further one of increased security. In this I have been disappointed: the difference in the quality of the coal appears to be the reason;—in North Wales it is a free burning and bituminous coal, and makes little or no clinker, and therefore essentially different from what I have described the coal used in Cornwall to be. With the latter these boilers do not appear to afford steam freely; whilst the brick sides of the fire-

place are so rapidly encrusted with clinker, and the door so frequently kept open to cleanse them, that much of the effect of the fire is destroyed.

The third class of boilers which have been used in the mines is that which includes Mr. Woolf's invention of a series of tubes filled with water and exposed to the fire. These boilers were the subject of one of his patents, and various descriptions of them are to be found in works which treat on these subjects. If one objection to them could be surmounted, they would probably be the best description of boilers we know of, but this has caused the use of them to be discontinued;—the tubes by expanding and contracting not only injure the joints, which must necessarily be numerous, but by sudden influences of the fire the water is displaced in some of them, and the tubes are injured and burst. No other inconvenience has occurred from this than what is occasioned by the frequent repairs thus called for; but it amounts of itself to a serious evil.

Of four accidents by the bursting of steam boilers which have come more under my notice as having occurred in mines where I am interested, and in the last two or three years, I would remark that the boilers were all of the first description. In other respects the circumstances differed very much. They were erected under the superintendence of different engineers,—were made by different manufacturers in parts of the country distant from each other, of materials from various sources; they were mostly nearly new or not apparently the worse for wear, and were each furnished with a safety-valve and gauge cocks; though I admit that there is not so much attention to the care of these matters in the boiler-houses of mines as could be wished.

The first accident was at Wheal Fortune, to one of six boilers which are employed to work the large engine there of 90-inch cylinder. I do not recollect that any thing remarkable occurred to observe upon with regard to this; the injury was limited to the boiler itself, and it occasioned no particular discussion. The engineer was Mr. Woolf.

The next was extraordinary from the circumstance of two boilers blowing up at the same moment or nearly so. This happened at Polgooth Tin Mine, where three were employed in the same house to work the engine (80-inch cylinder). The engine had been stopped a short time for some repairs to the pump-work in the shaft; but it seemed clear after the accident, by the most accurate investigation that could be made, that the steam had not acquired any formidable degree of pressure, nor was the water so low as to endanger the tube being improperly heated. The engineer was Mr. Sims, and the boilers

as

as well as the engine were nearly new. One man, unfortunately, was killed, and the stack of the engine-house was much shattered, as well as the building itself. The interior tubes of the boilers were much contorted and rent. Captain Reed, who was near the spot, remarked, that the one explosion was heard a little before the other, but the noise had hardly ceased when the second took place.

Some time after, one of the boilers of the 64-inch cylinder engine at East Crennis Mine blew up. This engine was also under the care of Mr. Sims, but had been much longer at work than that at Polgooth. The inner tube was compressed as if the fire had softened the part above it, though there did not appear to be any other reason to think that the water was too low. The ends were torn to pieces, and the tube was projected out of the case and out of the house, while the case itself remained in its place, and scarcely injured. No person was materially hurt.

The last accident, which has led more particularly to these remarks, happened at the Mold Mines in Flintshire, to a boiler of a similar construction; one of three working the Pen-ŷ-fron engine 66-inch cylinder, erected by Captain Francis the principal agent, but of late under the care of Mr. Bawden, engineer.

The outer case remained in its seat uninjured, as at East Crennis, and even the weight on the lever of the safety-valve was not disturbed; the inner tube was not moved out of its place, although it was very much flattened or compressed for a great part of its length, but in a contrary direction to that at East Crennis; the sides as it were having come together, and not the top and bottom, they approached so close to each other as to hold a brick, which it is not easy to account for being there. The part which contained the fireplace, and for some length near it, remained in the original form. The ends both here and at East Crennis presented an appearance as if they had broken the angle iron rather by the contraction of the tubes than by being pressed outwards.

Circumstances rendered it possible to get better evidence of the state of the steam and water, &c. than happens in most such instances; and it seemed certain that the former did not exceed a pressure of 30lbs. an inch, and that the other was quite at its proper height. There was a lead plug indeed above the fire which would have been destroyed if it were not so.

The engine had been stopped a few minutes; the engineman had opened the fire-doors of the three boilers, and had closed the dampers of the other two: he was on this boiler, putting down the damper in the flue, which was no sooner done than he observed a gust of flame rushing from the fire-

 place,

place, and almost immediately after an explosion, which made him jump from a door-way considerably above the level of the ground below, as the engine stands on the side of a steep hill:—this door was used to discharge the cinders from the ashpits. He alighted on the heap, and escaped out of the way just before the hot water gushed out. Two other men who were in the boiler-house were not so fortunate, and they were killed instantly by the boiling water; no mark of any other injury being to be found on their bodies.

In this case, had the rush of flame from the fireplace any thing to do with the subsequent explosion?

And admitting that the steam was so far within the pressure that could by mere expansive force regularly exerted injure such a boiler,—might not the rupture be occasioned by the aid that a vacuum suddenly created might produce?

Does not the bursting of the one boiler after another as at Polgooth, seem to indicate that exterior causes operated?

Is it possible to conceive,—supposing the pressure equal in two boilers as was the case at Polgooth, both being connected to the same steam-pipe,—that the relative strength of the two should be so exactly the same as that what would by mere expansive force burst the one should have the same effect upon the other?

Have not all calculation and reasoning with respect to the strength of boilers hitherto had regard merely to such expansive force uniformly exerted; and if we suspect or admit the action of concussion, or the effects that any thing like a blow would exert, ought we not to make a very different estimate in their construction?

My intention was rather to state the facts than to attempt an explanation of what is certainly at present very obscure; but that I may do all in my power to direct attention to the subject, I will venture on a supposition. At the Pen-ŷ-fron engine we see that the fire-door is thrown open, and then the current of air up the flue is stopped by closing the damper; the interior is filled with atmospheric air mixed to a certain extent with coal gas; the latter is increased by the distillatory action of the fire until the proportion is attained which is explosive; it takes fire, producing the rush of flame which would be followed by a sudden vacuum in the tube; while the other side, pressed by the steam, gives way to this sudden impulse, and is destroyed by a force very much smaller than would be required if uniformly exerted.

By some it has been suggested that hydrogen may have been generated by the decomposition of water from leaks in the boiler.

That

That sudden inflammations of gas in the chimneys of these engines does take place is, I believe, sufficiently obvious. By night it is observable that bursts of flame suddenly illuminating the surrounding scene, and rising to a considerable height above the summit of the stack, are seen to emerge, and after a blaze of some minutes diminish and retire into the flue, leaving all once more in perfect darkness. This effect I certainly do not recollect to have noticed where the coal is less bituminous. The fact is not, perhaps, of much importance; but it has been remarked upon by some who have witnessed the accident I have described, and has been discussed by them in reference to it, and therefore it is right to mention it.

Though I have been led to describe the bursting of boilers where what is called high-pressure steam has been used, I by no means think that boilers are safer because the steam in them is supposed to be limited to a lower degree of expansive force. High-pressure boilers are or ought to be very strong, and can only give way by a great increase of force beyond what they are calculated to resist, which should happen but seldom. Low-pressure boilers are from their construction very weak, and a little carelessness raises the power of the steam within them to the bursting point, and when they give way the consequences are often very fatal. Not to mention other instances, I may remark, that about twelve months since one of the old spherical construction, which is still much used in some parts of the kingdom, burst at a mine in Flintshire about 7 miles from the Mold Mines, and occasioned the death of 16 persons: it was replaced by two smaller boilers of the second kind I have described, and high-pressure steam applied with good effect to the engine, and with perfect security.

XXX. *On the Crystalline Forms of Wagnerite.* *By* A. Levy, *Esq. M.A. F.G.S.**

ONE of the rarest species in mineralogy is that which has been named Wagnerite, the chemical composition of which according to the analysis of Fuchs it as follows:

Phosphoric acid	41·73
Fluoric acid	6·50
Magnesia	46·66
Oxide of iron	5·
Oxide of manganese	0·50
	100·39

A large and well-defined crystal of this substance is preserved in Mr. Heuland's private collection, and it is the only

* Communicated by the Author.

one

one he has ever seen. It was sent to him still adhering to the matrix, but unfortunately by some accident has been detached from it. As no determination has been hitherto given, as far as I know, of the primitive form of this substance, the description of this crystal may not be uninteresting.

The form it presents is rather complicated, being the combination of fourteen modifications, and containing when complete fifty planes. Fig. 1. is a representation of it, but differs in appearance from the crystal, which offers only one of its summits, and in which the planes belonging to the same modification are of unequal extent. The simplest primitive form, from which I find fig. 1. may be derived, is an oblique rhombic prism, (fig. 2.), the lateral planes of which would correspond to the faces m, and the base to the face p of fig. 1., the ratio between one side of the base and the lateral edge being determined on the supposition that the planes e^1 result from a decrement by one row on the angles e of the base. Referred to such a primitive form the crystallographical sign of fig. 1. is

$$\mathrm{P}\, m\, g^3\, h^1\, h^5\, e^1\, e^{\frac{1}{2}}\, a^{\frac{1}{2}}\, b^{\frac{1}{2}}\, b^{\frac{1}{4}}\, a_3\, a_5\, c_3\, (b^1\, d^{\frac{1}{3}}\, g^2).$$

All the planes of the crystal are sufficiently brilliant to allow the use of the reflective goniometer to measure their in-

Fig. 1.

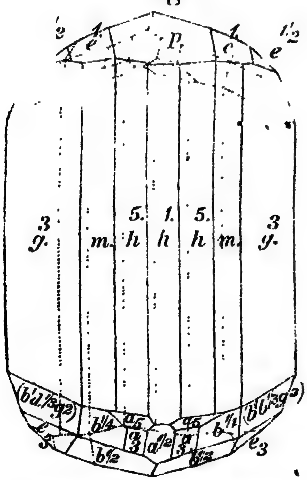
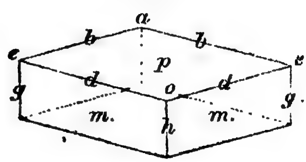


Fig. 2.



cidences, but the most brilliant are those of the modification g^3 and of the modification $(b^1\, d^{\frac{1}{3}}\, g^2)$; and it is from the measures of their incidences that the dimensions of the primitive and the other angles have been calculated. The agreement between the observations and the calculation has been in every instance

instance within half a degree. The parallelisms of edges exhibited by the figure, as well as those depending on the indices of the planes, but which cannot be shown by the drawing, have been verified with the reflective goniometer.

$$\mathrm{P}, m = 109^\circ\ 20' \qquad m, m = 95^\circ\ 25'$$

$$b : h :: 1 : 0{\cdot}264.$$

Plane angle of the base . . . . = 89° 1′
Plane angle of the lateral faces = 108 37.

$$\mathrm{P}, h^1 = 116^\circ\ 35' \qquad m, h^1 = 137^\circ\ 42'\ 30''$$

$$h^5, h^5 = 117\ \ 32 \qquad m, h^5 = 168\ \ 56\ \ 30.$$

$\mathrm{P}, g^3 = 102^\circ\ 27'$	$g^3, g^3 = 57^\circ\ 35'$	
$\mathrm{P}, e^1 = 161\ \ 23\ \ 30''$	$\mathrm{P}, e^{\frac{1}{2}} = 146\ \ \ 3.$	
$\mathrm{P}, a^{\frac{1}{2}} = 135\ \ 18$		
$\mathrm{P}, b^{\frac{1}{2}} = 150\ \ 30$	$m, b^{\frac{1}{2}} = 100\ \ 10$	$b^{\frac{1}{2}}, b^{\frac{1}{2}} = 138^\circ\ 53'.$
$\mathrm{P}, b^{\frac{1}{4}} = 125\ \ 18\ \ 30''$	$m, b^{\frac{1}{4}} = 125\ \ 21\ \ 30''$	$b^{\frac{1}{4}}, b^{\frac{1}{4}} = 108\ \ 49.$
$\mathrm{P}, a_1 = 132\ \ \ 0\ \ 30''$	$m\ a_3 = 116\ \ 18\ \ 30$	$b^{\frac{1}{4}}, a_3 = 164\ \ \ 6.$
$\mathrm{P}, a_5 = 112\ \ 45$	$m, a_5 = 136\ \ 29$	$a_5, a_5 = 117\ \ 39.$
$\mathrm{P}, e_3 = 138\ \ \ 3$	$m, e_3 = 110\ \ 39$	$g^3\ e_3 = 119\ \ 31.$

$\mathrm{P},(b^1 d^{\frac{1}{3}} g^2) = 114\ \ 30.$ $m,(b^1 d^{\frac{1}{3}} g^2) = 133\ \ \ 6$ $g^3,(b^1 d^{\frac{1}{3}} g^2) = 143\ \ 32.$
$g^3,(b^1 d^{\frac{1}{3}} g^2) = 129\ \ 15.$ $b^1,(b^1 d^{\frac{1}{3}} g^2) = 160\ 32.$

The perpendicular drawn from the angle o on the opposite edge h meets it at a distance from a equal nearly to $\frac{5\,h}{2}$.

There is some indication of cleavage in a direction parallel to the modification h^1.

I took the specific gravity, and I found it, the water being at a temperature of nearly 60°, equal to 3·01. Which agrees very nearly with that obtained by Fuchs, which is 3·11.

The fracture in a direction transverse to the prism is uneven and splintery.

It is easily scratched with a knife, and the streak is white.

The colour, transparence, and lustre of the crystal I have described, are perfectly similar to those of a Brazilian topaz, with which substance, Wagnerite has been formerly confounded.

The planes of the prism are strongly striated, with the exception of the planes g^3. All the other planes are without striæ, and more or less brilliant.

The locality is the valley called Höllgraben, near Werfen in Salzburg, where it is found in small veins of quartz in clay slate. Beudant mentions the United States as another locality of the same substance.

XXXI. *On*

XXXI. *On Captain* Parry *and Lieut.* Foster's *Experiments for ascertaining the Velocity of Sound at Port Bowen. By* William Galbraith, *Esq. M.A.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

IN the first Number of the New Series of the Philosophical Magazine and Annals of Philosophy, there is a paper (page 12), purporting to be a reply from Captain Parry and Lieutenant Foster, to my remarks on the velocity of sound at Port Bowen; the meaning of which I do not exactly comprehend, as I think they *apparently* controvert my remarks, while at the same time they *indirectly* acknowledge the justice of them. In fact, my formulæ were investigated long before their experiments were made known. I cursorily noticed them at the end of the paper, when it was just upon the point of being transmitted to London, and I hope that my paper upon comparison will be found to be characterized with as much *fairness* and candour as theirs. Indeed, I spoke with as much courtesy of them in that paper as I did of Newton, and I do not think they deserve *more*.

However, if they will *demonstrate* the truth of the proposition which they have *enunciated* as following from their observations, under the Table of the velocity of sound at Port Bowen, page 86 of the Appendix to the Third Voyage, without any equivocation of language,—such as speaking of an *increased density*, instead of a *diminished elasticity*, &c.—I shall be most happy to correct any mistake or indelicacy of language which I may have inadvertently employed.

I am, gentlemen, your most obedient servant,

Edinburgh, Jan. 21, 1827. William Galbraith.

XXXII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Dec. 15.—THE reading of a paper was concluded, entitled "Additional Notes on the Opposite Coasts of France and England, including some Account of the Lower Boulonnois," by William Henry Fitton, V.P.G.S. &c.

Since the reading of a former communication of the author, the correct identification of the beds beneath the chalk suggested by Mr. Lyell, and an examination of the strata in the vicinity of Weymouth, have enabled him to compare some portions of the country on the opposite sides of the English Channel more accurately than before was practicable: and he now, 1st, describes in detail the strata which succeed the chalk in the vicinity of Folkstone; and 2dly, gives a general description of the Lower Boulonnois.

lonnois. The following table exhibits the series of beds within the tracts just mentioned.

Names, chiefly derived from locality in England.	Places of Occurrence. In England.	Places of Occurrence. In the Lower Boulonnois.
Chalk	Cliffs from Dover to Folkstone-hill—Beachy-head to Brighton—I. of Wight—I. of Purbeck, Dortsetshire, &c.	Coast from Sangatte to Blancnez,—and thence on the boundary of the Lower Boulonnois, to Mont St. Frienx, &c. W. of Neufchatel.
Merstham Stone (Greensand—Fire-stone.—Upper-greensand.—*Tufau* of the French.)	Merstham, Reygate, and Godstone Firestone-pits—Western Sussex (Malm Rock)—I. of Wight—Swanage Bay, I. of Purbeck, &c.	Coast between Blanc-nez and Wissant.
Gault .... (Folkstone-marl.—*Dieve* of the French.)	Copt-Point, N. of Folkstone.—Valley beneath the chalk in Kent, Surrey, and Sussex.	Coast on the N. of Wissant.—vicinity of Hardinghen,—Lottinghen,—vicinity of Samer.
Shanklin Sands (lower Greensand.—*Tourtia* of the French.)	Coast Folkstone to Hythe.—vicinity of Maidstone, Kent.—Western Sussex.—Shanklin and Black-gang chines, I. of Wight.	Coast N. of Wissant—vicinity of Wissant.—Wooded hills parallel to the chalk, from Desvres to Samer, &c. &c.
Weald Clay .	Wealds of Kent and Sussex.—Cowleaze-chine, I. of Wight.—N. of Swanage Bay, I. of Purbeck.	Not ascertained in the Boulonnois.
Hastings Sands..	Hastings, Sussex.—S. coast of the I. of Wight—Swanage Bay.	
Purbeck Stone .	I. of Purbeck—Summit of the I. of Portland.	Qu. traces in the upper part of the cliffs from Gris-nez to Equihen.
Portland Stone...	Shotover Hill, and Garsington, Oxfordshire—Brill-hill, Bucks.	Upper part of the cliffs from Gris-nez to Equihen.—Mont Lambert quarries.
Kimmeridge, and *Weymouth beds*	Coast near Weymouth,—Hedington quarries, Oxfordshire.	Coast from Gris-nez to Equihen.—Mont Lambert quarries,—Vicinity of Desvres—of Samer, &c. &c.
Pisolite and *Coral-rag*............	Coast near Weymouth.	Basinghen, Hautenbert, Alinctun,—Hesdin L'Abbé, &c.; Vicinity of Samer.
Oxford Clay......	Coast near Weymouth.—vicinity of Oxford.	Vicinity of Wast—Houlfort.—Between Basinghen and Marquise.
Bath-oolite	Vicinity of Bath.	Vicinity of Marquise—Quarries at Leubringhen—Ardentun—Rety, &c.
—(unc-	onformable)—	
Coal-formation		Vicinity of Hardinghen,—Lochinghen,—Cedur, &c.
Mountain-Limestone.	Derbyshire.—Devon.—Vicinity of Bristol.—Dublin.	Leulinghen,—Quarries at Ferques, Haut-banc, &c.

I. On the N.E. of Folkstone, the chalk is succeeded by the equivalent of the *Merstham Firestone*, (or green sand,) which is there however not more than fifteen or sixteen feet in thickness; and this is followed immediately by *Gault*. The *Shanklin Sands*, (or lower green sand,) which come next in succession, are composed of three groups, which may be recognized also in the interior of the country.—The first and uppermost consisting of sand, abounding in irregular concretions of limestone and chert, sometimes disposed in courses oblique to the general direction of the strata. And the top of this formation, in the vicinity of Folkstone and Hythe, forms an extensive plateau resembling that of the Blackdown range of hills in Devonshire.—The second number of the group likewise consists chiefly of sand, but in some places so much mixed with clay, or with oxide of iron, as to retain water; and it is remarkable for the great variation of its colour and consistency,—from the state of loose bright yellow or ferruginous sand, to that of a dark greenish tough mass, like that of the cliffs of Shanklin and Black Gang-chines, which correspond to it in geological situation.—The third and lowest group of the Shanklin Sands abounds, near Folkstone, much more in stone, the concretional beds being closer together and more nearly continuous; and the fossils of this group, which are very numerous, agree with those of the corresponding beds in Sussex, the Isle of Wight, and Devonshire. The sections of the *Weald Clay*, and *Hastings Sands*, being imperfectly displayed on the coasts of Kent and Sussex, the author gives detailed lists of the beds at Cowleaze-chine, &c., on the south coast of the Isle of Wight, and on the shore of Swanage Bay in the Isle of Purbeck, where these formations are fully disclosed; referring for an account of the geological relations of those tracts, to a paper published by himself in the Annals of Philosophy for November 1824*.

II. The Lower Boulonnois may be described as constituting a flattened dome of unequal curvature, surrounded on three sides by an amphitheatre of chalk, which has been removed by denudation from the whole of the interior: the lower strata having a very gentle inclination where they emerge from beneath the chalk, but rising from the sea at a much more considerable angle. From the *chalk* down to the *Shanklin* (or lower-green) *sands*, the strata of the opposite coasts near Calais and Folkstone, precisely correspond; and the same beds may be traced beneath the chalk, almost without interruption, around the whole of the denudation: the *gault* especially, being very distinctly disclosed in the vicinity of Hardinghen, where it is succeeded by the Bath oolite, and by the coal formation. The next succeeding beds of the English coast, *Weald-clay* and *Hastings sands*, (which it is remarkable, have not yet been found in the interior of England,) appear to be wanting also in the Boulonnois; or, if they do exist there, to occupy a very small space. But some traces of the lowest members of the group to which these two strata belong, and which is remarkable from its containing throughout the remains of freshwater shells, are visible on the

* New Series, vol. viii. p. 365, &c.

summit

summit of the cliffs between Gris-nez and Equihen; where a thin bed occurs of somewhat bituminous clay, abounding in silicified wood, the cavities of which are coated with minute crystals of quartz. This bed corresponds precisely to that which exists on the top of the Isle of Portland, bearing there the name of 'Dirt,' and abounding in similar wood; and on the French coast it is associated with beds of limestone, different from the stone beneath, and containing shells in great numbers, apparently of the genera Cyclas and Ampullaria. The next stratum of the Boulonnois is the equivalent of that form of the *Portland limestone* which occurs at Garsington and Shotover Hill in Oxfordshire, and at Brill and other places in the vicinity of Aylesbury in Buckinganishire:—respecting its geological relations, however, some doubts still remain to be cleared up; several of the fossils being the same with those of the Isle of Portland, but the aspect of some of the beds a good deal different. The formation in the Boulonnois consists, as in Oxfordshire, of calcareous concretions of great size, abounding in petrifactions, and imbedded in yellowish somewhat ferruginous sand:—and the appearances of the stratum, especially between Gris-nez and Audreselles, where the shore is covered with these enormous masses fallen from the sands above, is exceedingly striking and remarkable. To this formation a series of beds succeeds, the equivalent of the strata between the Portland limestone and the coral rag;—corresponding precisely to those of the shore near Weymouth, and consisting of alternations of sand, limestone and clay, in some instances bituminous and abounding in fossils.—These occupy the whole of the lower part of the cliffs from Gris-nez to Equihen, and are visible in several places in the interior. The *pisolite and coral rag* are not seen upon the coast, but come up at a short distance within it; and their outcrop is conspicuous at Basinghen, and along a line extending from that place, by Wierre and Hautenbert, to Alinctun. On the north of that line this formation is succeeded by a valley constituting a very remarkable feature of the country, and occupied by beds of clay containing fossils identical with those of the *Oxford clay*, and including, especially at the lower part, subordinate beds of sand and calcareous grit. These are followed on the north, near Marquise, by the equivalent of the *Bath oolite*, (the upper members of the oolitic series, Cornbrash and Forest marble, being indistinct or wanting):—and this formation seems to come in without any intervention, immediately after the gault or subjacent sand, on the north of the denudation; where it occupies the surface, in nearly horizontal strata placed unconformably over beds of the *coal formation*, or of *mountain limestone*.—The former of these is disclosed in a small space only, in the vicinity of Hardinghen: and the author refers for an account of it to a Memoir now preparing for publication by M. Garnier of Arras.—The *mountain limestone*, which is the lowest formation of the Boulonnois, in some places comes in immediately after the lower greensand, or the gault, without the intervention even of the oolite: and near Landrethun the distance from the chalk to the limestone beds is not more than a quarter of a mile. In some cases, when

T 2 the

the incumbent mass of oolite is removed, the surface of the limestone beneath is found to be smooth, or slightly waved like the sands of the shore after the tide has retired; and the rock is pierced by tubular perforations evidently the work of marine animals; a proof that the surface must have been exposed to their activity for some time before the oolite was deposited. The beds of mountain limestone of the ordinary character, in some places alternate with dolomite, precisely resembling that which is found in the same geological situation near Dublin. And the fossils of this formation in the Boulonnois are the same with those of Derbyshire, Gloucestershire, and Dublin.

On comparing in a general view the strata of the opposite coasts, it will be seen that those of the Boulonnois do not occur upon the English shore, except in the vicinity of Weymouth; and if the line of elevated strata which extends from that part of the coast of Dorsetshire, through the Isle of Purbeck and the Isle of Wight, were continued to the eastward, it would reach the French coast near Gris-nez;—just at the place where the same beds arise, and where it is remarkable their position is likewise very highly inclined.

Jan. 5.—A notice was read, accompanying some specimens from the Hastings Formation, with a copy of a work on the fossils of Tilgate Forest; by G. Mantell, Esq. F.R., L. and G.S.,—in a letter to R. I. Murchison, Esq. Sec. G.S., F.R.S. &c.

The author states that his principal object in the present volume, is to give a correct and extended view of that division of the Hastings Sands, distinguished by him in the strata of Tilgate Forest, the relations of which he illustrates by the section of a quarry at Pounceford, where the Ashburnham limestone with bivalves, &c. is seen overlying sandstone and calciferous grit (Tilgate stone).

A recapitulation of the animal and vegetable remains (in which the author particularly notices that gigantic Saurian the *Iguanodon*) shows the vast preponderance of land and freshwater exuviæ in the Hastings strata over those of marine origin; a circumstance in strict accordance with what is now constantly occurring in all deltas and estuaries of great rivers.—A description is given in the concluding chapter of the work, of the probable condition of the country anterior to the epoch of this deposit.

The reading of a paper was commenced, entitled "On the coal-field of Brora, in Sutherlandshire, North Britain, and upon some other secondary deposits of the North of Scotland;" by R. I. Murchison, Esq. Sec. G.S., F.R.S. &c.

ASTRONOMICAL SOCIETY.

Dec. 8.—There was read a letter from Mons. Flaugergues, of the observatory at Viviers, on a Comet discovered there March 29th, 1826. M. Gambart, of Marseilles, had informed M. Flaugergues, that he had discovered, on the 20th of March, a comet, which shortly afterwards moved to the constellation Taurus.—M. F. on looking for that comet, perceived under the left arm of Orion, a white round nebulosity, scarcely visible, which he supposed

posed was the comet announced by M. Gambart. Looking for the same comet on subsequent evenings, the only one of his instruments through which it was visible, was an achromatic telescope whose double object-glass was of 40 inches and 6 lines focal distance, and 30 lines and a half of aperture;—this telescope has an eye-glass of 2 inches focal distance, and is of admirable clearness. It is mounted on a parallactic foot, but required the appropriation of a micrometer, before it could be well applied to this class of observations. On the 4th, 5th and 6th of April, he made some good observations on the comet with this apparatus: on the 7th he could not find it, nor has he seen it since. He had no suspicion that this was any other comet than the one observed by M. Gambart, until he compared his own observations with those recorded in No. 4, vol. xiv. of Zach's *Correspondance Astronomique*, from which he at once saw that his observations and M. Gambart's related to two different phænomena. M. Flaugergues has communicated his observations on the evenings of the 4th, 5th, and 6th of April; and though they are nearer together than could be wished, he has deduced from the corresponding geocentric places, the following elements of the orbit, regarded as parabolic:

Inclination	9° 32′ 26″
Longitude of the ascending node	193 31 11
Longitude of perihelion	222 53 32
Perihelion distance	0·646146

Passage of perihelion, April, 26·95973; or
26th of April, $23^h\ 2^m\ 0^s$ M.T. at Viviers.

Motion direct.

It is only in this last element, that this comet resembles the comet announced by M. Gambart.

There was next read a letter from M. Gambart to the Foreign Secretary, dated Marseilles, 29th of October 1826, announcing his discovery, the preceding evening, of a Comet having then $14^h\ 38^m$ Æ, and 36°·1 of Dec. North.

From a subsequent communication to different astronomers, dated Marseilles 22d of November, which was also read, it appears that M. Gambart from observations of the 7th, 8th, 9th, and 10th of November, computed the elements of the orbit, and thence deduced a curious anticipatory result. He gives for

Passage of the perihelion Nov.	18·8085 M.T. from midn^t.
Perihelion distance	0·02314
Longitude of perihelion	314° 57′ 28″
Longitude of the ascending node	236 9 54
Inclination	89 59 24

Motion retrograde.

From these elements, it was anticipated that the comet would transit the sun's disc on the 18th of November, and that

The Immersion would take place at	$5^h\ 25^m$ A.M.
Passage through the node	7 1
Shortest distance of comet from ☉'s centre	2′ 40″
Emersion of comet from disc	8 38

The expected phænomenon was not observed in England.

There

There was next read a letter to the President from Professor Santini, dated Padua, 6th November 1826, and containing various astronomical observations. M. Santini commences by detailing observations of a Comet discovered by M. Pons at Florence, the 7th of October. These observations extend from 29th of August to 5th of November. From those of 30th of August, 28th of September, and 20th of October, M. Santini has computed the following elements, for a parabolic orbit; though the true orbit he says would rather seem to be hyperbolic.

Passage of the perihelion	October $9^{d}\cdot2310$ M.T. at Padua.
Log. perihelion distance	9·93028
Longitude of perihelion	57° 35′ 6″
Longitude of the node	43 9 5
Inclination	25 30 7

Motion direct.

M. Santini communicates, 2dly, Observations of the planet Ceres near its opposition to the Sun in 1826, viz. from 26th of June to 1st of July inclusive. 3dly, Observations of the planet Pallas near its opposition in 1826, viz. from 26th of June to 29th inclusive. 4thly, Observations of the planet Vesta near its opposition in 1826, viz. from August 13th to 23d inclusive. M. Santini has compared these observations with the geocentric positions of Pallas and Vesta, as computed by Professor Encke, and published in Bode's *Jahrbuch* for 1828, pages 157, 160. The mean differences are, for Pallas in Æ $+ 3''\cdot96$, in decl. $-0\cdot54$: for Vesta in Æ $+ 11''\cdot43$ in decl. $-4''\cdot32$.

In a postscript dated 7th of November, M. S. announces the discovery of another Comet by M. Pons, in the constellation Bootis, on the 22d of October.

Lastly, There was read a letter from Colonel Beaufoy to Lieut. Stratford, R.N., containing Observations on the solar eclipse of November 28th, made by Lieut. G. Beaufoy, R.N., Bushey Heath.

Nov. 28th. Solar eclipse, Began $21^{h}\ 46^{m}\ 4^{s}$; M.T., Bushey.
Ended 23 58 19, do. do.

No spot visible on the sun's disc:—Edge of the moon uneven. Her horns blunted.

XXXIII. *Intelligence and Miscellaneous Articles.*

CHLORINE IN THE NATIVE BLACK OXIDE OF MANGANESE.

MR. J. MACMULLEN, in a paper contained in the last number of the Royal Institution Journal, has arrived at some very extraordinary conclusions.—He considers not only that native black oxide of manganese contains chlorine, "but that it is in the state of chloric acid, and that the native oxide is, at least in part, and probably in proportions varying with the different specimens of the ore, a native chlorate of manganese."

I shall probably offer a few observations on the above paper in the next number of the Phil. Mag. and Annals. R. P.

PHOS-

PHOSPHORUS IN KELP.

•Repeated trials, we are told by Van Mons, have proved, that the roundish and longish veins found in the *varec-soda* or kelp, after the matter soluble in water has been removed, are principally composed of phosphorus.—*Jameson's Edin. Journal, Jan.* 1827.

How did the phosphorus escape combustion?

DECOMPOSITION OF OXALIC ACID BY SULPHURIC ACID.

In our last number we noticed M. Dumas's experiment of the decomposition of oxalic acid into carbonic acid and oxide by means of sulphuric acid and binoxalate of potash. I have repeated the experiment successfully, and the same decomposition is effected when oxalic acid is substituted for the binoxalate of potash.—R. P.

PHOSPHORESCENT FLUOR SPAR.

At a recent meeting of the Philomathic Society of Paris, a specimen of fluor spar was exhibited by M. Becquerel, which had been found in granite, in Siberia, and sent by M. Leman: it shines in the dark when warmed, with a remarkably strong phosphorescent light, increasing as the temperature is raised. The light augments when it is plunged in water: and in boiling water the spar becomes so luminous that the letters of a printed book could be seen near the glass vessel containing it. On boiling mercury it emitted such a light as to enable a person to read printing at a distance of five inches. M. Eyries mentioned at the same meeting, the statement of Sir John Mandeville, that at the entrance of a town in Great Tartary were two columns surmounted by stones which shone brightly in the dark.—*Royal Institution Journal, Jan.* 1827.

In Phillips's Mineralogy, p. 172, a fluor spar is mentioned as occurring in Siberia which is of a pale violet colour, and gives out, according to Pallas, white light by the heat of the hand merely; when subjected to the temperature of boiling water, it gave a green light. It would seem that this variety phosphoresces even at a lower temperature than that exhibited by M. Becquerel.—Compact phosphorescent fluor has been found also in Cornwall.

PERKINS'S HIGH-PRESSURE ENGINES.

A Report has been made by M. Girard, on a memoir by Sir William Rawson, on Perkins's High Pressure Engines, read to the Academy of Sciences at Paris. The report, after enumerating the advantages stated in the memoir to be possessed by the apparatus, observes, how desirable it would be that these assertions should be supported by authentic experiments, which it would appear to them they want at present, unless it be in the propulsion of balls, which comprises the whole of the official proofs.—*Bull. Univ.*—*Royal Institution Journal, Jan.* 1827.

FORMATION OF OLEIC AND MARGARIC ACIDS FROM FAT.

MM. Bussy and Lecanu treated soft fat with four times its weight of concentrated and boiling nitric acid: after an hour's action, the mixture

mixture was allowed to cool, and the fatty matter which floated upon the fluid was separated; it was of a canary yellow colour, inodorous, and softer than the fat employed. It was perfectly washed with distilled water, so as to separate all matters soluble in it; it was then treated with alcohol, which dissolved it almost entirely. The undissolved portion appeared to be fat, probably a little altered. The alcoholic solution reddened litmus paper strongly; by evaporation in a salt-water bath, it gave a yellowish residual mass, which was pressed after having been placed between folds of blotting paper. By pressure a very acid yellow liquid was obtained, which combined with alcohol in all proportions, with solution of potash, and was susceptible of forming with barytes a compound insoluble both in alcohol and water.

The solid matter which remained between the folds of paper, was first shaken with hot barytes water, and the insoluble barytic salt was treated with boiling alcohol, in order to separate the unacidified fatty matter which it might retain. The alcohol dissolved a small portion of fatty matter, and left the barytic salt unacted upon: this salt being decomposed by dilute muriatic acid, yielded a solid fatty matter, which was washed with distilled water until the washings ceased to act upon solution of nitrate of silver or coloured re-agents.

When thus separated from all excess of muriatic acid, it was dissolved in alcohol and crystallized; it was colourless, inodorous, tasteless, and lighter than water; it melted at + 144° Fahr.; the alcohol while boiling dissolved it readily, and on cooling, fine pearly crystals were deposited. It reddened moistened litmus paper, combined readily with potash and barytes, forming with the first of these bases a compound analogous to common soaps, and like them soluble in water and alcohol; the barytic salt was pulverulent and insoluble in these fluids.

It results from the preceding experiments that fat treated with nitric acid is partly converted into oleic and margaric acids. As analogy of composition admits extending these results to all bodies formed of stearine and oleine, it will be observed that the property of converting these bodies into oleic and margaric acids, which for a long time was limited to the action of the alkalies only, then observed in sulphuric acid, in oxygen and in heat, is also found to exist in nitric acid. From these circumstances we are inclined to suppose that analogous phænomena are produced whenever the arrangement of the elements of oleine and stearine is disturbed.—*Journal de Pharmacie, Nov.* 1826.

SEPARATION OF THE COLOURING MATTER OF MADDER.

The *Société Industrielle* of Mulhausen (Département du Haut Rhin) have offered the following prizes:

1st. A prize of three hundred francs for a ready and easy method of determining the comparative values of different kinds of madder.

2d. A prize of six hundred francs, (to which a zealous member of the Society will add six hundred more,) for a process for separating the

the colouring matter of madder, and thus determining the quantity which a given weight of madder contains.

Memoirs and specimens, accompanied with a sealed paper containing the name of the author, to be addressed, (carriage paid) before the 25th of April next, to M. Isaac Schlumberger at Mulhausen, President of the Society.

BISMUTH COBALT ORE.

This mineral has hitherto been found only at Schneeberg in Saxony: for a knowledge of it we are indebted to M. Kersten of Göttingen.—*External Characters:* Colour intermediate between lead gray and steel gray; lustre metallic, and glistening or glimmering; texture radiated, partly stellular partly parallel. It scratches fluor spar, but this degree of hardness is occasioned by intermixed quartz. Streak dull, colour not changed, but the powder soils. Specific gravity = 4·5 — 4·7.—*Chemical Characters:* Before the blowpipe on charcoal gives out white vapours of arsenious acid; deposits on it a yellow crust, during which the ore becomes of a brown colour. When well roasted before the blowpipe, and then mixed with glass of borax and melted, it communicates to it a smalt blue colour. If some small pieces of the ore are exposed to a low red heat in a glass tube, it affords a considerable quantity of arsenious acid. It is composed of

Arsenic	77·9602
Cobalt..................	9·8866
Iron.....................	4·7695
Bismuth	3·8866
Copper	1·3030
Nickel	1·1063
Sulphur	4·9160
	99·9282

The characteristic ingredients of this ore are arsenic-cobalt, and arsenic-bismuth, a combination of these metals hitherto not met with in the mineral kingdom.—*Jameson's, Edin. Journ. Jan.* 1827.

ISERINE AND IRON SAND IN CHESHIRE.

Dr. Traill, of Liverpool, many years ago discovered the above substances at Seacourse opposite to Liverpool, loose on the beach and disseminated through a bed of crumbling sandstone which lies below the thick bed of loam which forms the Cheshire soil at that spot. He afterwards traced it along the shores of the Mersey for several miles, and has very lately traced it quite across the hundred of Wirral, in Cheshire, from the shores of the Mersey to those of the Dee.—*Jameson's Edinb. Journ. Jan.* 1827.

EXPERIMENTS ON CERTAIN OXALATES.

M. Sérullas finds that when dry and pure oxalate of potash, either acidulous or neutral, is finely powdered with an equal weight of antimony and heated in a forge fire for eight or ten minutes in a co-

vered crucible, there is always procured a button which is an alloy of potassium and antimony.

When well dried oxalate of lead mixed with very small portions of potassium, perfectly freed from naphtha, is put into the bottom of a glass tube, air being carefully excluded, by excess of the oxalate a violent detonation suddenly takes place, before the heat is sufficiently great to effect the decomposition of the oxalate, when no potassium is present. The tube is spotted with metallic lead, the potassium is oxidized, and there is no carbon deposited. An examination of the gas resulting from this instantaneous decomposition may elucidate the nature of the oxalates; but hitherto the apparatus employed has always been broken by the explosion. Oxalate of copper treated in the same way also occasions strong detonation, and metallic copper appears.—*Journ. de Pharm. Nov.* 1826.

SEIDLITZ POWDERS.

M. Planche, one of the editors of the *Journal de Pharmacie*, remarks that Dr. Paris in his *Pharmacologia* has given the composition of what are termed Patent Seidlitz Powders, as consisting of Rochelle salt, bicarbonate of soda, and tartaric acid. And M. Planche, after stating the surprise of M. Robinet that any powders so named should contain no sulphate of magnesia, proposes the following formula as an improvement:

Take of Sulphate of magnesia, in fine powder, ʒij
Bicarbonate of soda . . . . ℈ij
Mix carefully, and mark it Powder No. 1.
Tartaric acid in fine powder, gr. xl.
Mark it Powder No. 2.

There then follow the usual directions for mixing in water and taking during effervescence.

Previously to giving this improved formula, M. Planche favours us with the following tirade against the English Government, for suffering Patent Seidlitz Powders to be sold without containing sulphate of magnesia: "The English Government, which receives a considerable duty from all secret remedies, takes but little trouble to inquire whether the remedy is new, efficacious or dangerous; it is sufficient that it be productive to the revenue, to allow of its being authorized." Does not M. Planche know that the spirit of quackery is the same on both sides of the Channel! For whose benefit, but that of the apothecaries, is the *Codex medicamentarius* authorized by the French Government, stuffed with such disgusting preparations as frog, lizard, viper, and snail broth?

One word more to M. Planche. In his short notice he has had occasion to mention two English names, and he has made two ridiculous blunders. First we have "*la Pharmacologie du Dr. John Ayrton*, Paris, édition de 1820." By thus using italic type for *Dr. John Ayrton*, and roman type for Paris, with a comma between them, M. Planche's countrymen may suppose Dr. John Ayrton's work had been printed at Paris, instead of the author's name being John

John Ayrton Paris. The second blunder occurs in mentioning a quack remedy, which is described as *white head's essence of mustard.*

JET DISCOVERED IN WIGTONSHIRE.

Beautiful specimens of this mineral have been found between a bed of peat and yellow clay, in the peninsula formed by Loch Ryan and the Irish Channel, by Sir Andrew Agnew.—*Jameson's Edinb. Journ. Jan.* 1827.

ORIGIN OF THE DIAMOND.

In the Philosophical Magazine for November 1826, is inserted an article from the Asiatic Researches, vol. xv., "On the Diamond Mines of Southern India," by the late Mr. Voysey, Geologist to the Trigonometrical Survey of India. The question, as to the proper matrix of the diamond, being one of great interest in the inquiry concerning the periods in the physical history of the earth, at which the various forms of carbon were respectively first produced, and also in that relating to the natural process by which this gem was formed, and as the paper alluded to appears to be less decisive on the subject than could be wished, it may be useful to future inquirers to make a few remarks on Mr. Voysey's statements.

The description given in this paper of the constitution of the Nalla Malla mountains is somewhat indecisive and confused; and does not afford the means of determining, with precision, to what formation the sandstone-breccia belongs, which is stated to be the matrix of the diamonds. But it would appear that this range consists of transition strata, intersected or overlaid in some places, by traprocks; the breccia, of course, belonging to the former class. The imperfect nature of this description, however, is rather to be attributed to the existing deficiency of correct general views on the geology of India, than to any want of accuracy or of knowledge in the observer.

But an aggregate rock, consisting, it is stated, of jasper, quartz, chalcedony, and hornstone, cemented together by a quartzose paste, and passing into a pudding-stone composed of rounded pebbles of the same substances, cemented by an argillo-calcareous earth, cannot be the *matrix* of the diamond, as Mr. Voysey supposes. The diamonds it contains, like the other minerals of which it is constituted, must have existed previous to its consolidation; and it is possible, that, also like them, they were derived from the destruction of some older rocks. The matrix of the Brazilian diamond, we have some reason to believe, from a specimen not long since described by Mr. Heuland, (Geol. Trans. 2d series, vol. i. p. 419,) is a brown iron-stone, which occurs in thick veins or beds resting on chlorite slate; and from the disintegration of which and the accompanying rocks, the materials of the alluvial conglomerate called *cascalhão*, from which the diamonds of Brazil are usually obtained, may be supposed to have resulted.

Professor Jameson, a few years since, expressed an opinion, that the diamond, being a form of pure carbon, might have been originally secreted from the juices of some plant, in a manner similar to

 that

that in which silica is deposited in the joints of certain arundinaceous vegetables. And Dr. Brewster, at a later period, (See Edin. Phil. Journ. vol. iii. p. 100.) from the very compressible state which certain optical characters of the diamond evince it to have once possessed, and which, he states, could not arise from the action of heat, nor exist in a mass formed by aqueous deposition, inferred, that the diamond probably originates, like amber, from the consolidation of perhaps vegetable matter, which gradually acquires a crystalline form.

It is not intended, in the present remarks, to advocate the opinion either of Professor Jameson or Dr. Brewster, though, under all the circumstances of the case, both are deserving of attention; but Mr. Voysey quotes the observations of the latter writer, with the intent to prove, that, as "diamonds have for two centuries at least been found in a *rock*, generally supposed to owe its origin to deposition from water," his inference can apply, only, to the diamonds found in alluvial soil. It is unnecessary to show, that if Mr. V. had really discovered the matrix of the diamonds, the same origin must have been attributed to those specimens which occur in alluvial soils; but as the rock in which he states the diamonds of Southern India are found, consists of the re-united debris of former rocks, his observations throw no further light on the history of this substance, than as indicating, perhaps, as the breccia is probably one of great antiquity, that this form of carbon existed at an early period in the formation of the present crust of the earth. This, indeed, we may likewise infer, perhaps, from the matrix of the diamond mentioned by Mr. Heuland. It might also be concluded from that specimen, that the diamond is an original production of the mineral kingdom. Still, however, this cannot be decided from a single instance of its occurrence in a mineral vein; and the remarkable analogies between amber and the diamond, discovered by Dr. Brewster, besides that already mentioned, certainly entitle his opinion on its origin to philosophical examination. It must be remarked, at the same time, that when Dr. Brewster published his observations, the diamond had been found only in alluvial deposits; but the discovery of it in a matrix of ironstone, (if confirmed by future discoveries,) and also in a breccia of ancient formation, removes Dr. B.'s objection to supposing the former compressible state of the diamond to have arisen from the action of heat, which is deduced merely "*from the nature and the recent formation of the soil*" in which only, at the time his paper appeared, it had been found.

Perhaps the opinions on this subject of Professor Jameson and Dr. Brewster are not incompatible with each other; and if the diamond were really produced in a manner resembling that in which Tabasheer is formed, we might expect to find some analogies in the physical structure of the two substances; though, on the other hand, the absence of such analogies would not negative the proposition, as the chemical nature and functions in the vegetable world of carbon and silica are so widely different. Dr. Brewster has examined the structure of Tabasheer; but whether it exhibits any analogies to that

that of the diamond the writer of this notice is not aware; nor has he, at present, the means of ascertaining.

It is worthy of remark, whilst considering this interesting question, that the observation of Newton, which has usually been cited merely as a sagacious conjecture of the combustible nature of the diamond, is more precisely an anticipation of the conclusion, to which the optical characters of this gem have led Dr. Brewster. Sir Isaac infers, from the great refractive power of the diamond, "that it is probably *an unctuous body coagulated*." (See Phil. Mag. vol. xvii. p. 197.) Such an inference, derived from an optical character entirely distinct from that on which Dr. Brewster founds his opinion, affords a strong additional reason for examining this view of the question.

The present state of the inquiry, then, respecting the origin of the diamond, appears briefly to be as follows:

Argument in favour of the vegetable origin of the diamond:

This gem is a form of carbon: of that species of matter which constitutes the solid basis of all vegetable productions.

It is *certainly known* to occur in nature, only in alluvial deposits, and in a conglomerate rock containing fragments of older rocks; the evidence of its existence in mineral veins being confined to a single specimen, and of course doubtful. Therefore we have no *proof* that the diamond is originally a mineral substance.

Sir I. Newton inferred that it was an unctuous body coagulated:—Professor Jameson suggests the probability of its vegetable origin:—Dr. Brewster has ascertained that it presents analogies with amber, a substance which we have every reason to believe is of vegetable origin, and also, that, like amber, it must once have been in an extremely soft and compressible state; whence he is led to conclude, that it originates from the slow consolidation of perhaps vegetable matter.

Argument in favour of the mineral origin of the diamond:

The occurrence of carbon in the forms of anthracite and graphite in mica-slate and other rocks believed to be of primary formation, appears to indicate that this species of matter is an original element of the mineral kingdom, and existed anterior to the production of vegetables; and that, therefore, the carbon of the diamond is not *necessarily* derived from vegetables.

A specimen of diamond has been obtained, having for its matrix a species of ironstone, known to occur in veins or beds on chlorite slate; a rock of early transition or probably primitive formation; whence *it may be inferred* that this substance is an original product of the mineral kingdom.

The compressible state once possessed by the diamond, may (for aught that has been advanced to the contrary,) have resulted from the action of heat; and, if so, the optical characters discovered by Dr. Brewster, do not necessarily refer its origin to the vegetable kingdom.

E. W. B.

HARBOUR OF KO-SI CHANG.

The geographical position of the cluster of islands which form the harbour of Ko-si Chang renders them of some importance to navigators,

navigators, and particularly to Europeans trading to Siam. Although these islands possess a fine and convenient harbour, and lie within four hours' sail of the mouth of the Siam river, they are but little known, and there is not even a correct chart of them extant. We have therefore the pleasure of laying before our readers such information regarding them as we have been able to collect.

The group is situated in latitude 13° 12′ North, and longitude 100° 55′ East, and about twenty-six miles from the mouth of the river of Bankok, from which they bear about S.E. The nearest part of the main is the high land of Bampesoi, which is only a few miles distant. They are seven or eight in number, but with the exception of the two largest, called by the Siamese Ko-si Chang and Ko Cram, they are small and unimportant.

Ko-si Chang, the largest of the cluster, is about seven miles long and three broad, and is composed of hills of considerable height, clothed to the water's edge with trees. The varieties of wood are numerous, and some of the descriptions, such as maple and sissoo, are well suited for fine work. The trees are not, however, found of sufficient height or dimensions for ships' masts or yards. On this island there is no cultivation, except a small spot which is inhabited by a solitary Chinese. Ko Cram is about one-fourth the size of the large island, and has a small village on one end of it, occupied by Siamese fishermen, by whose industry a considerable portion of the island has been cleared of wood and brought into cultivation, and produces abundance of maize, and such vegetables as are common on the continent.

These islands are famous for some rare and beautiful varieties of the wild pigeon. The most remarkable are a large white species with the tips of the wing and tail black, found on most of the islands in the Gulf, but unknown on the continent; a beautiful brown and purple-coloured description, which is very rare; and one or two varieties of the small green pigeon. There is a large root found close to the sea, on the smaller islands, which appears to be a new species in the list of plants. In appearance it has a close resemblance to the *Dioscorea bulbifera*, or common yam, but it has little or no taste, and grows to an enormous size. We have seen a specimen of this root, which measured ten feet circumference, and weighed 474lbs. The natives use it as a medicine, for which purpose it is prepared by cutting it into thin slices and drying it in the sun, when it is pounded or ground down into a powder of a light brown colour. This powder is administered in cases of fever, agues, &c. Land-crabs are numerous in several spots throughout the islands, and are eaten by the natives.

The Cochin-Chinese who visit Ko-si Chang, on their voyages to Siam, have erected a temple on the large island. This is a small white building, and stands conspicuous on an eminence at the S.W. end. Their traders touch here regularly for supplies of water and fire-wood; the latter of these articles is easily procured, and is taken away in large quantities by them on their return to Cochin-China, in some parts of which country wood is a scarce article.

The shores afford the edible birds'-nests, so much in request amongst

amongst the Chinese; but they are of inferior quality, probably owing to their being permitted to remain on the rocks from season to season. Rock-oysters are also very abundant, and a few sea-slugs, or beech-de-mer, are found, but not in sufficient quantity to render them worth collecting. Stone ballast for the use of ships is obtained with ease and without danger to the boats.

The harbour which is formed by the two large islands is well sheltered, and affords anchorage for almost any number of vessels, and protection from the wind and sea in every direction, except to the northward; but from this quarter the sea cannot affect it much, on account of its vicinity to the shoals at the head of the Gulf. The best entrance is from this quarter, but there is also a passage to the southward between the islands. The holding ground is tolerably good, but it will always be necessary for ships to ride with chain cables, owing to the roughness of the bottom, which is in many parts covered with stones. The rise and fall of water is considerable, being about ten feet at spring tides, and the tide runs strong through the harbour. On the S.W. end of the large island there is a fine stream of fresh water at which a hundred casks may be filled in one day. The stream issues from the hill, and escapes to the sea in a small sandy bay, finding its way under the bank of sand which lines the beach. The Cochin-Chinese temple already mentioned is erected on the hill from which the stream flows.—*Singapore Chron.—Asiat. Journ.*

RIVERS OF ASSAM.

We learn from this quarter that a further attempt at geographical investigation was lately made to the north-east, but was stopped short by want of supplies. It has been ascertained, however, that for 100 miles from Suddeya, the Brahmaputra pursues an easterly course. The Braham Kund is now said to be, not the source of the Brahmaputra, but of a small stream which falls into it. There is great reason to think that the Dihong will prove to be the San-po; it is a large stream, three times the size of the Brahmaputra, although, at the same time, its depth of water is scarcely sufficient for a river that has passed through so lengthened a course; and Buchanan's suggestion may not be altogether devoid of probability, that the San-po falls into a lake, of which the Dihong is one of the outlets. This would account for the tradition current amongst the Assamese, that about ninety years ago the river came down with a prodigious increase of its waters, and deluged the country. The Dihong, in the cold season, discharges about 50,000 cubic feet of water in a second.

The next inquiries, we understand, are to be made in the direction of the Bor Kampti country, which lies about the sources of the Irawadi, about latitude 27° 28′.

The weather upon the Brahmaputra, before the junction of the Dihong, was very cool, the thermometer being not unfrequently, in the beginning of last month, below 70°, and the temperature of the river was 61° in the morning. The river rises and falls very suddenly: there is nothing but jungle on both banks.—*Cal. Gov. Gaz. May.* 15.—*Asiat. Journ.*

NEW

NEW PATENTS.

To —— Thomas, of Vale Grove, Chelsea, esquire, for his process of rendering boots, shoes, and other articles water-proof.—Dated the 22d of December 1826.—6 months allowed to enrol specification.

To David Redmund, of Greek-street, Soho, engineer, for improvements in the construction and manufacture of hinges.—22d of December.—6 months.

To Elijah Galloway, of the London Road, Surrey, engineer, for a rotary steam-engine.—29th of December.—6 months.

To John Whiting, of Ipswich, architect, for his improvements in window sashes and frames.—9th of January 1827.—2 months.

To James Frazer, of Houndsditch, engineer, for an improved method of constructing capstans and windlasses.—11th of January.—6 months.

To James Frazer, of Houndsditch, engineer, for an improved method of constructing boilers for steam-engines.—11th of January.—6 months.

To William Wilmot Hall, of Baltimore, America, at present residing in Westminster, for an engine for mooring and propelling ships, boats, carriages, mills and machinery of every kind.—15th of January.—2 months.

To William Hobson, of Markfield, Stamford Hill, Middlesex, for an improved method of paving streets, lanes, roads and carriage ways in general.—15th of January.—2 months.

To James Neville, of New Walk, Shad-Thames, engineer, for an improved carriage, to be worked or propelled by means of steam.—15th of January.—6 months.

To William Mason, of Castle-street East, Oxford-market, Westminster, patent axletree-maker, for improvements in the construction of those axletrees and boxes for carriages known by the names of Mail-axletrees and boxes.—15th of January.—2 months.

To Robert Copland, of Wilmington-square, Middlesex, for improvements on a patent already obtained by him for combinations of apparatus for gaining power.—16th of January.—15 months.

SCIENTIFIC BOOKS.

Just Published.

The Miner's Assistant, containing Instructions for surveying Mines, and Works connected therewith: with Tables for facilitating their various calculations. By R. Thomas, civil engineer, Falmouth.

Preparing for Publication.

A new Work, by G. Poulett Scrope, Esq. F.R. and G.S.S. "On the Geology of Central France, and particularly the Volcanic Formations of Auvergne, the Velay, and Vivaray," in 4to. accompanied by an Atlas, containing numerous coloured Plates, and two large Maps, will be published in a few days.

Results

Results of a Meteorological Journal for the Year 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

By WILLIAM BURNEY, *LL.D.*

Latitude 50° 47′ 20″ North—Longitude 1° 7′ West of Greenwich. In time 4′ 28″.

1826. Months.	Barometer. Max.	Min.	Media.	Range.	No. of Changes.	Spaces described.	Greatest Variation in 24 hours.	Media at 8 A.M.	Media at 2 P.M.	Media at 8 P.M.
	In.	In.	In.	In.		In.	In.	In.	In.	In.
January	30·51	29·54	29·980	0·97	21	3·55	0·34	29·978	29·973	29·983
February	30·44	29·33	29·957	1·11	26	6·00	0·68	29·950	29·957	29·963
March ...	30·36	29·37	29·958	0·99	23	6·11	0·47	29·949	29·958	29·966
April ...	30·37	29·18	30·010	1·19	15	5·24	0·97	30·004	30·003	30·019
May ...	30·28	29·64	30·013	0·64	30	3·24	0·34	30·021	30·011	30·005
June ...	30·48	29·84	30·230	0·64	19	2·82	0·24	30·234	30·229	30·227
July ...	30·37	29·60	29·982	0·77	23	3·67	0·40	29·988	29·986	29·975
August	30·35	29·60	29·979	0·75	21	4·15	0·28	29·988	29·984	29·969
Septemb.	30·24	29·20	29·890	1·04	18	5·44	0·67	29·883	29·892	29·898
October	30·17	29·42	29·904	0·75	28	4·65	0·56	29·899	29·891	29·912
Novemb.	30·49	28·60	29·776	1·80	17	7·98	0·90	29·785	29·769	29·766
Decemb.	30·50	29·18	29·849	1·32	25	5·56	0·56	29·847	29·840	29·853
Averages for 1826.	30·51	28·60	29·961	11·97	266	58·41	0·97	29·960	29·958	29·961

1826. Months.	Self-registering Thermometer. Max.	Min.	Media.	Mean Range.	Gt. Var. in 24 hours	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Mean Temp. of Spring Water.	De Luc's Hygrometer. Max.	Min.	Mean Range of the Index.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Media at 8,2,&8 o'cl.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January	49	17	35·56	32	17	38·94	33·87	35·23	50·10	93	59	34	74·0	80·1	78·9	77·7
February	56	33	45·91	23	16	49·32	44·28	45·64	49·44	96	63	33	75·8	84·4	84·2	81·5
March ...	59	31	45·56	28	21	50·35	44·16	44·55	49·44	95	50	45	64·2	72·3	71·6	69·4
April ...	68	33	51·90	35	23	58·10	50·77	48·73	49·05	96	42	54	58·1	68·0	70·8	65·6
May ...	74	38	55·18	36	28	61·84	54·35	53·16	49·50	65	49	16	54·0	58·9	60·6	57·8
June ...	86	50	65·28	36	28	72·53	65·23	63·40	50 73	47	28	19	37·2	40·3	41·0	39·5
July ...	81	51	66·84	30	27	73·42	67·42	64·90	52 47	61	46	15	52·5	55·3	56·4	54·7
August	83	51	67·47	32	22	74·29	67·32	65·48	54·02	98	40	58	51·5	59 9	65·1	58·9
Septemb.	74	48	61·33	26	20	66·37	63·23	59·27	55·09	100	43	57	58·6	70·0	73·9	67·5
October	68	38	56·26	30	21	60·35	56·06	54·26	55·34	100	44	56	64·8	74·1	77·4	72·1
Novemb.	59	29	44·88	30	20	48·93	43·03	44·47	54·30	98	49	49	65·7	73·2	75·2	71·4
Decemb.	57	32	46·43	25	15	49·00	44·93	46·00	52·68	100	60	40	72·5	78·4	80·9	77·3
Averages for 1826.	86	17	53·55	30·2	28	58·62	52·64	52 09	51·85	100	28	39·7	60·7	67·9	69·7	66·1

TABLE (continued.)

Scale of the Winds.

1826.	North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.
January ..	3	10	3	4	3	2½	½	5	31
February	...	...	2	4	5½	10½	4	2	28
March ...	3	10	2	3	...	6	2½	4½	31
April	6	1½	½	3½	½	4	6½	7½	30
May	10	10½	1	4	½	1	2	2	31
June ...	7	7	2	3½	½	2	1	7	30
July	4	2½	½	6	2	11	3	2	31
August ..	2	3	2	4½	5	10	2	2½	31
September	2	5	3	4	3	6½	2½	4	30
October ..	1	1	4½	4	1	7	5	7½	31
November	8	5	1	1	1	4	3	7	30
December	3	5½	2	3	4	3	4½	6	31
Averages for 1826.	49	61	23½	44½	26	67½	36½	57	365

Modifications of Clouds.

1826.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.
January ..	11	8	27	1	9	20	13
February	18	9	27	...	17	18	21
March ...	13	5	24	1	17	23	15
April	17	12	29	3	24	20	12
May	17	9	24	...	24	20	16
June ...	23	16	21	...	23	20	7
July	20	15	28	...	24	20	14
August ..	26	17	29	1	26	18	13
September	20	10	26	...	24	18	17
October ..	16	9	27	2	20	27	16
November	17	7	29	...	17	16	17
December	11	3	30	...	11	14	18
Averages for 1826.	209	120	321	8	236	234	179

Weather.

1826.	A clear Sky.	Fair, with Clouds.	An overcast Sky.	Foggy.	Rain, &c.	Total Number of Days.
January ..	5	10	12	1	3	31
February	3	11	6	½	7½	28
March ...	5	13	9	...	4	31
April	3	14	10	...	3	30
May	7	13	7	...	4	31
June ...	8	16	4½	...	1	30
July	7	16	5½	...	2½	31
August ..	5½	18	4½	...	3	31
September	5	12	6½	½	5½	30
October ..	3	13	10	...	5½	31
November	5	10½	7½	½	6	30
December	1½	9	13	½	7	31
Averages for 1826.	58	156½	95½	3	52	365

Atmospheric Phænomena.

1826.	Anthelia.	Parhelia.	Paraselenæ.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.	Evaporation in Inches, &c.	Rain in Inches, &c.
January ..	...	1	...	1	3	...	2	...	...	1·00	0·890
February	...	1	...	1	1	...	3	...	...	1·30	3·860
March ...	...	2	...	2	2	1	3	...	...	3·52	2·615
April	1	2	2	4	1	...	8	1	...	3·05	1·000
May	1	2	...	3	...	1	5	2	1	4·60	2·275
June ...	1	...	...	3	...	1	2	3	...	6·00	0·895
July	...	1	...	2	1	...	11	2	2	4·35	1·605
August ..	...	...	2	2	3	1	80	6	2	4·60	1·520
September	...	...	...	2	1	3	8	1	...	2·70	4·555
October ..	...	2	...	1	3	4	14	3	1	1·60	2·225
November	...	3	...	1	1	2	5	1	...	1·15	3·640
December	...	...	...	...	1	...	2	...	...	0·75	2·935
Averages for 1826.	3	14	4	22	17	13	143	19	6	34·62	28·015

ANNUAL RESULTS FOR 1826.

Barometer.

	Inches.
Greatest pressure of the atmosphere, January 17th, Wind S.	30·510
Least ditto ditto Nov. 13, Wind N.	28·600
Range of the quicksilver	1·910
Annual mean pressure of the atmosphere	29·961
Mean pressure for 201 days with the moon in North decl.	29·953
———— for 183 days with the moon in South decl.	29·932
Annual mean pressure at 8 o'clock A.M.	29·960
———— at 2 o'clock P.M.	29 958
———— at 8 o'clock P.M.	29·961
Greatest range of the quicksilver in November	1·800
Least range of ditto in May and June	0·640
Greatest annual variation in 24 hours in April	0·970
Least of the greatest variations in 24 hours in June	0·240
Aggregate of the spaces described by the rising and falling of the quicksilver	58·410
Number of changes	266

Self-registering Day and Night Thermometer.

	Degrees.
Greatest thermometrical heat, June 27th, Wind W.	86
———— cold, January 14th, Wind E.	17
Range of the thermometer between the extremes........	69
Annual mean temperature of the external air	53·55
———— of do. at 8 A.M.	52·64
———— of do. at 8 P.M............	52·09
———— of do. at 2 P.M...........	58·62
Greatest range in May and June	36·00
Least of the monthly ranges in February	23 00
Annual mean range	30·20
Greatest monthly variation in 24 hours, in May and June..	28·00
Least of the greatest variations in 24 hours, in December..	15·00
Annual mean temperature of spring water at 8 o'clock A.M.	51·85

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the atmosphere, several times in September, October and December	100
Greatest dryness of ditto, June 23rd	28
Range of the index between the extremes	72
Annual mean state of the hygrometer at 8 o'clock A.M. ..	67·9
———— at 8 o'clock P.M. ..	69·7
———— at 2 o'clock P.M. ..	60·7
———— at 8, 2 & 8 o'clock ..	66·1
Greatest mean monthly humidity of the atmosphere, in Feb.	81·5
———— dryness of ditto in June	39·5

Position

Position of the Winds.	Days.
From North to North-east	49
—— North-east to East	61
—— East to South-east	23½
—— South-east to South	44½
—— South to South-west	26
—— South-west to West	67½
—— West to North-west	36½
—— North-west to North	57
	——365

Clouds, agreeably to the Nomenclature, or the Number of Days on which each Modification has appeared.

	Days.		Days.
Cirrus	209	Cumulus	236
Cirrocumulus	120	Cumulostratus	234
Cirrostratus	321	Nimbus	179
Stratus	8		

General State of the Weather.	Days.
A transparent atmosphere without clouds	58
Fair, with various modifications of clouds	156½
An overcast sky without rain	95½
Foggy	3
Rain, hail and sleet	52
	——365

Atmospheric Phænomena.	No.
Anthelia, or mock-suns opposite the true sun	3
Parhelia, or mock-suns on the sides of the true sun	14
Paraselenæ, or mock-moons	4
Solar halos	22
Lunar halos	17
Rainbows, solar and lunar	13
Meteors of various sizes	143
Lightning, days on which it happened	19
Thunder, ditto ditto	6

Evaporation.	Inches.
Greatest monthly quantity in June	6·00
Least monthly quantity in December	0·75
Total amount for the year	34·62

Rain.	
Greatest monthly depth in September	4·555
Least monthly depth in January	0·890
Total amount near the ground for the year	28·015
Total amount near 23 feet high for ditto	25·770

N.B The Barometer is hung up in the Observatory, 50 feet above the low-water mark of Portsmouth Harbour; and the Self-registering Horizontal Day and Night Thermometer, and De Luc's Whalebone Hygrometer, are placed in open-worked cases in a northern aspect out of the rays of the sun, 10 feet above the garden ground. The Pluviameter

Pluviameter and Evaporator have respectively the same square area: the former is emptied every morning at 8 o'clock, after rain, into a cylindrical glass-gauge accurately graduated to $\frac{1}{100}$dth of an inch; and the quantity lost by evaporation from the latter, is ascertained at least every third day.

Barometrical Pressure.—The mean altitude of the Barometer this year very nearly coincides with that of last year, and in these two years it is unprecedented in our register, arising from a more settled state of the atmosphere in the summer and winter months. The *maximum* pressure is not so high as that of last year by $\frac{3}{10}$ths of an inch; but the *minimum* pressure is exactly the same.

The aggregate of the spaces described by the alternate rising and falling of the quicksilver, is 24·71 inches less than in 1824, and 8·82 inches less than that of last year.

For 201 days in which the moon ranged in North declination, the mean pressure was $\frac{1}{50}$th of an inch higher than in the 183 days in which she ranged in South declination.

Temperature.—The annual mean temperature of the external air is $\frac{54}{100}$ths of a degree higher than that of last year, and 1·42 degree higher than the mean of the last ten years. As it respects the temperature of the atmosphere there were two singular deviations from the regular course of the seasons this year: namely, the mean temperature of February was about one-third of a degree higher than that of March; and the mean temperature of December was $1\frac{1}{2}$ degree higher than that of November. The mean temperature of spring water at 8 o'clock A.M. this year, is 1·70 degree lower than the mean temperature of the external air ten feet from the ground; and for the last six years it is 1·09 degree lower. With the exception of the genial year 1822, this year has been the warmest that we have experienced for a great number of years past.

Wind.—During the first four months, and in September, we had a long continuance of brisk and hard gales; the other part of the year, particularly the last three months, very few gales prevailed. The number of strong gales, or the days on which they have prevailed this year, are as in the following scale.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
9	21	2	2	1	21	0	4	60

Those from the N.E. and S.W. points of the compass are equal; and both together, more than two-thirds of the whole number; and they are diametrically opposite.

In comparing the scale of the ordinary winds, it appears to coincide nearly with the scale for last year, except from the West points, the loss on which seems to have gained upon the North wind. Here the prevailing wind for many years past is decidedly from the South-west and West points, as influenced by our local position with respect to the Western Ocean.

Weather.

Weather.—The number of clear natural days, or days on which no clouds have appeared, as stated in the table, is three more than last year; and the number of rainy days three less: the number of fair days with clouds is $9\frac{1}{2}$ less, and the number of overcast days without rain, $10\frac{1}{2}$ more than last year. The year has been distinguished for a seasonable winter, a warm and healthy spring, a hot and fruitful summer, and a humid and rather a sickly autumn from the great and rapid changes in the temperature and state of the atmosphere. Even in the winter and autumn, only a sufficient quantity of rain fell to keep the ground in a growing condition. From the close of March to the end of August only $7\frac{1}{3}$ inches fell here, which with the hot summer months, caused a drought, materially shortened the grass, stunted the barley generally, and on high and light soils the wheat, but not the fruit crops: the produce of the earth, therefore, met with advantages and disadvantages. So dry a period we have not experienced since the year 1818; and the annual depth of rain at the ground coincides nearly with the dry year 1820.

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1826.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·50 Dec. 28. Wind N.W.—Min. 29·18 Dec. 1. Wind W.
Range of the mercury 1·32.

Mean barometrical pressure for the month	29·849
——— for the lunar period ending the 28th instant	29·781
——— for 15 days with the Moon in North declination . .	29·685
——— for 15 days with the Moon in South declination . .	29·877
Spaces described by the rising and falling of the mercury . . .	5·560

Greatest variation in 24 hours 0·560.—Number of changes 25.

Therm. Max. 57° Dec. 10 & 11. Wind S.—Min. 32° Dec. 21. Wind N.
Range 25°.—Mean temp. of exter. air 46°·43. For 29 days with ☉ in ♈ 45·91
Max. var. in 24 hours 15°·00—Mean temp. of spring water at 8 A.M. 52°·68.

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the morning of the 7th . . .	100°
Greatest dryness of the air in the afternoon of the 4th . . .	60
Range of the index	40
Mean at 2 P.M. 72°·5—Mean at 8 A.M. 78·4—Mean at 8 P.M.	80·9
—— of three observations each day at 8, 2, and 8 o'clock . .	77·3

Evaporation for the month 0·75 inch.
Rain near ground 2·935 inches.—Rain 23 feet high 2·720 inches.

Summary of the Weather.

A clear sky, $1\frac{1}{2}$; fine, with various modifications of clouds, 9; an overcast sky without rain, 13; foggy, $\frac{1}{2}$; rain, 7.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
11	3	30	0	11	14	18

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	$5\frac{1}{2}$	2	3	4	3	$4\frac{1}{2}$	6	31

General

General Observations.—The first part of this month was very wet, and the latter part dry (excepting a day or two), with more healthy airs. It has been remarkably mild for the season, the greatest difference in the state of the thermometer between the days and nights being only 15 degrees, and in three of the nights it rose higher than in the days.

The mean temperature of the external air this month, is upwards of four degrees higher than the mean of December for the last ten years; nor have we had so mild a December since that in 1821: indeed, it is one and a half degree higher than that of last month (November), which in this place is unprecedented. The Barometer too, has been tolerably steady; and it appears from the scale of the winds that they were nearly equal in duration from the eight points of the compass.

There having been but a few frosty mornings this month, the temperature of spring water is therefore nearly three-quarters of a degree higher than at this time last year.

In the nights of the 10th and 11th, several small rings of colours, and a close corona within a yellow discus halo, appeared round the moon, which were succeeded by rain.

Although a water-spout is said to have burst over Bungay, in Suffolk, on the 3rd instant, yet that day was fine here.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one lunar halo, two meteors, and two gales of wind, namely, one from the East, the other from South-west.

London.—Twelfth month. 1. Cloudy: rain at night. 2. Fine. 3. Cloudy and fine. 4. Cloudy. 5. Cloudy: snow in the night. 6. The ground covered with snow in the morning, which soon disappeared, rain coming on. 7. Rainy morning: day rainy. 8—11. Cloudy. 12. Rainy. 13. Cloudy. 14. Fine. 15—17. Cloudy. 18, 19. Gloomy. 20. Fine. 21. Very fine. 22. Morning foggy: day fine. 23. Gloomy. 24. Drizzling. 25, 26. Gloomy. 27. Fine. 28. Very fine. 29—31. Fine.

RESULTS.

Winds, N. 1: NE. 3: E. 1: SE. 8: S. 1: SW. 4: W. 4: NW. 8.

Barometer mean height for the month	30·074 inch.
Thermometer, mean height for the month	42·258°
Evaporation	·89 inch.
Rain	1·61 inch.

Penzance.—Dec. 1. Rain. 2. Showers. 3—4. Showers, hail and rain. 5—7. Rain. 8. Fair. 9, 10. Rain. 11. Showers. 12. Fair. 13. Fair: rain. 14, 15. Misty. 16—18. Fair. 19, 20. Rain. 21. Fair. 22. Clear. 23—13. Fair.—Rain gauge at the *ground level.*

RESULTS.

Barometer, mean height	29·69
Register Thermometer	44°
Rain-gauge at the ground-level	1·875
Prevailing wind	N.W.

Boston.—Dec. 1. Fine: rain P.M. 2. Cloudy: rain P.M. 3. Fine: rain P.M. 4—6. Fine. 7, 8. Cloudy. 9. Fine. 10, 11. Cloudy. 12. Fine. 13—15. Cloudy. 16. Rain. 17—20. Cloudy. 21, 22. Fine. 23—25. Cloudy. 26. Fine. 27. Cloudy, 28. Fine. 29. Cloudy. 30, 31. Fine.

Meteor-

Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1826.	Barometer.						Thermometer.						Hyg. Gosp.	Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosp. 8½ A.M.	Bost. 8½ A.M.	London.		Penzance.		Gosp. 8½ A.M.	Bost. 8½ A.M.		Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.			Max.	Min.	Max.	Min.													
Dec. 1	29·68	29·38	29·28	29·20	29·50	29·20	44	37	50	41	40	37	92	SW.	S.	SW.	calm	...	...	·21	...	0·250	...
2	29·38	29·34	29·23	29·20	29·18	28·80	50	37	50	40	45	40	74	SW.	NW.	W.	W.	...	...	...	...	·020	·07
3	29·64	29·34	29·43	29·40	29·47	29·10	42	30	50	44	40	38·5	74	SW.	NW.	W.	W.	...	0·05	·01	...	·010	·09
4	29·90	29·64	29·56	29·54	29·41	29·16	46	35	48	43	36	33	68	NW.	NW.	NW.	W.	...	...	...	...	...	·04
5	29·91	29·90	29·61	29·55	29·74	29·50	39	33	56	44	40	33·5	71	NW.	W.	W.	calm	...	...	...	·405	1·080	...
☽ 6	29·91	29·80	29·54	29·54	29·60	29·50	50	35	58	48	46	33·5	92	SE.	SW.	N.	calm	...	·10	·39	...	·100	...
7	29·80	29·50	29·48	29·33	29·65	29·30	53	48	58	48	54	50·5	100	W.	SW.	SW.	calm	...	...	·30	...	·270	·16
8	30·07	29·45	29·40	29·30	29·32	28·92	52	38	56	46	51	47	80	W.	W.	SW.	W.	...	...	·03	...	·020	·04
9	30·07	30·06	29·43	29·40	29·83	29·52	53	39	56	46	42	42	90	W.	S.	N.	W.	...	·05	·15	...	·185	·10
10	30·06	30·05	29·52	29·50	29·80	29·53	54	48	56	49	53	49·5	90	SE.	SW.	S.	calm	...	...	·05	·200	·035	·03
11	30·05	30·00	29·50	29·50	29·79	29·50	55	44	55	48	51	51	83	SE.	W.	S.	calm	...	...	·02	...	...	...
12	30·00	29·74	29·48	29·30	29·76	29·50	51	47	54	48	51	42·5	78	S.	W.	S.	calm	...	·05	·22	·070	·070	...
13	29·79	29·74	29·28	29·26	29·47	29·22	52	34	54	46	49	47	83	SE.	W.	S.	calm	...	...	...	...	·090	...
○ 14	29·81	29·79	29·30	29·29	29·56	29·27	44	42	54	45	46	46	86	SE.	W.	S.	SE.	...	...	...	...	·410	...
15	29·79	29·78	29·34	29·30	29·55	29·35	46	44	52	45	48	45·5	68	SE.	N.	SE.	calm	...	·05	·05	...	·160	...
16	30·06	29·79	29·40	29·35	29·49	29·40	44	38	54	45	49	45·5	88	E.	N.	E.	r.	...	...	...	...	...	·29
17	30·18	30·06	29·76	29·70	29·73	29·61	45	40	52	44	47	43·5	74	SE.	N.	NE.	calm	·49	...	...	...	...	...
18	30·25	30·18	29·76	29·70	29·92	29·70	42	38	52	44	42	40	72	NE.	NW.	NE.	calm	...	·10	...	...	...	...
19	30·25	30·00	29·72	29·70	30·00	29·73	41	39	50	44	42	40·5	65	SW.	SE.	NE.	calm	...	...	...	...	·010	...
20	30·00	29·85	29·52	29·45	29·82	29·65	43	37	50	45	45	39·5	80	SE.	W.	SE.	calm	...	...	·15	...	·215	...
21	30·42	30·00	29·80	29·70	29·64	29·45	40	28	48	45	40	35	77	NW.	N.	NW.	NW.	...	·05	...	1·150	...	·25
☾ 22	30·42	30·41	30·08	29·98	30·20	30·00	44	30	46	38	32	33	67	W.	N.	NW.	calm	...	...	...	...	...	...
23	30·47	30·42	30·10	30·08	30·20	29·97	47	40	48	40	48	46	84	NW.	W.	NW.	calm	...	...	...	...	...	...
24	30·51	30·47	30·12	30·10	30·24	30·02	45	42	50	44	47	46	82	N.	N.	NW.	calm	...	·15	...	...	...	...
25	30·60	30·51	30·15	30·12	30·27	31·10	45	40	50	44	46	45·5	70	N.	N.	N.	calm	...	...	·03	...	·010	...
26	30·69	30·60	30·25	30·20	30·36	30·17	42	39	48	42	45	41·5	71	NE.	N.	NE.	calm	...	...	...	...	...	·06
27	30·71	30·69	30·30	30·30	30·44	30·30	45	30	48	40	44	40	68	NE.	NE.	E.	calm	...	·10	...	...	...	...
● 28	30·71	30·58	30·36	30·30	30·47	30·30	37	29	44	36	40	35·5	70	NW.	NE.	N.	calm	...	...	...	...	...	...
29	30·58	30·38	30·30	30·28	30·40	30·10	47	37	51	46	41	41	80	NW.	NW.	NW.	calm	...	...	...	...	...	...
30	30·44	30·38	30·20	30·18	30·22	29·88	49	44	50	46	48	42	70	NW.	N.	NW.	W.	...	...	...	...	...	...
31	30·40	30·16	30·12	30·00	30·22	29·87	48	43	52	46	45	44·5	84	NW.	N.	W.	calm	·40	·05	...	0·050	...	...
Aver.:	30·71	29·34	29·72	29·67	29·847	29·63	55	28	52	44	44·93	41·8	78·4					·89	0·75	1·61	1·875	2·935	1·13

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

MARCH 1827.

XXXIV. *Biographical Notice of M.* Piazzi.*

THE sciences have recently lost Joseph Piazzi; he died at Naples the 22d of July 1826. He was born at Ponte, in the Valteline, on the 16th of July 1746; he took the garb of the theatins at Milan, and finished his novitiate in the convent of St. Anthony. In his studies, which were conducted successively at Milan, Turin, and Rome, he had the advantage of being under the tuition of Tiraboschi, Beccaria, Leseur, and Jacquier. Intending to engage himself in a similar course of teaching, he went as professor of philosophy to Genoa, where expressing his opinions too freely, he alarmed the zeal of the Dominicans, who would have disturbed his tranquillity if the grand-master Pinto had not engaged him to teach mathematics with him in the new University of Malta. On the suppression of this body, Piazzi went to Rome, and afterwards to Ravenna, where he occupied the philosophical and mathematical chair at the College of the Nobles: he there made himself enemies by publishing some philosophical theses which appeared to be too bold as coming from a young monk. He was nevertheless thought worthy to succeed the preacher at Cremona, where he had retired after the theatins had given up the management of the college at Ravenna. He was appointed reader on theological dogmas at Saint-André *della Valle*, at Rome, where Father Chiaramonte (Pius VII.) was his colleague, and who retained for him on the throne, the same sentiments which he had expressed in the cloister. In 1780, Piazzi, by the advice of Father Jacquier, accepted the pro-

* *Bulletin des Sciences*, Nov. 1826, p. 341.

professorship of the higher mathematics at the Academy of Palermo. On his arrival there he reformed the method of teaching, by substituting modern institutes for the works of Wolff, and by rendering those of Locke and Condillac familiar, which were previously almost unknown. By his knowledge he powerfully contributed to dispel the darkness, which, under the combined influence of the Inquisition and the Jesuits, still enveloped the territory of Sicily. Not satisfied with having rekindled the love of letters, he obtained from the prince of Caramanico, the viceroy of the island, permission to establish an observatory at Palermo.

He visited France and England, in order to procure the instruments necessary for his new establishment, and to form an acquaintance with those astronomers who were most celebrated for their labours and knowledge. He was acquainted with Lalande, Jeaurat, Bailly, Delambre, and Pingré. He took advantage of the departure of Cassini, Méchain, and Legendre, who were deputed to determine the difference of the two meridians of Paris and Greenwich, to visit England, where he became intimate with Maskelyne, Herschel, and Vince, and especially with Ramsden, to whom he entrusted the construction of his instruments. He frequented the Greenwich observatory, and from it he observed the solar eclipse of 1788, of which he gave an account in a memoir inserted in the Philosophical Transactions.

Being desirous of avoiding the uncertainty which quadrants always leave in the mind of the observer, Piazzi engaged Ramsden to construct for him a vertical circle of five feet in diameter, accompanied with an azimuth, and divided with the precision of which that artist was then alone capable. He went every day to the workshop to hasten the work; and being dissatisfied with Ramsden's slowness, he conceived that he might stimulate his self-love by a letter addressed to Lalande, on the life and labours of this optician. The trick succeeded; in a short time after Piazzi had the satisfaction to see his great circle finished, and he also obtained a transit instrument, a sextant, and some other less important machines. The English minister pretended that the circle belonged to the class of discoveries, and consequently that it was subject to the prohibitory duties of England; but Ramsden protested that if it was a new invention the merit of it belonged to Piazzi, whose instructions he had merely executed. This declaration obviated every difficulty, and Piazzi returned to Sicily, carrying with him all the instruments. He put the new observatory in activity, and it was the most southern which then existed, that of Malta having been destroyed by fire in 1789.

1789. As soon as every thing was in order, observations were commenced, the results of which were published in 1792.

Piazzi immediately occupied himself with forming a new catalogue of stars, the exact position of which appeared to him as the only true basis of astronomy. François Lalande, in France; Cagnoli, in Italy; de Zach, Henry, Barry, in Germany; had partially commenced this work, relying upon the position of thirty-six stars which Maskelyne had pointed out to astronomers as fixed points of comparison. Piazzi, on the contrary, was unwilling to confide in a single observation: the slightest inaccuracy on the part of the observer, the smallest imperfection in the instruments, were accidents too probable to render them admissible. He also knew, that if Flamsteed, Mayer and Lemonnier had continued their observations, they would probably have deprived Herschel of the honour of his discovery. These considerations made him return many times to the same star, before he fixed its position, and it was according to this laborious but exact method that Piazzi finished his first great catalogue, containing 6748 stars, which was crowned by the Academy of Sciences of France, and which was welcomed by all astronomers. But a more interesting result of this system was the discovery of an eighth planet, which opened the way to new conquests in the heavens. On the 1st of January 1801, Piazzi, in examining the 87th star of the zodiacal catalogue of Lacaille, between the tail of Aries and Taurus, perceived a star of the 8th magnitude, which he occasionally observed. His habit of verifying the observations of the previous day caused him to remark, on the following, a difference in the place of the small star, which he at first took for a comet. He communicated his observations to Oriani, who, observing that this luminous point had not the nebulosity of comets, and that it remained stationary and retrograded, in the manner of a planet in a moderately short space, calculated it on the hypothesis of a circular orbit. He was not deceived in his hypothesis, which, confirmed by other astronomers, awarded to Piazzi the honour of the discovery. He gave it the name of *Ceres Ferdinandea:* Lalande was of opinion that it should be called simply *Piazzi.* The king of Naples was desirous of celebrating this event by a gold medal, struck with the effigy of the astronomer; but Piazzi, modest in his triumph, requested that the value of the present might be employed in purchasing an equatorial, which was wanting in his observatory. He continued, in the mean time, with perseverance, the works which he had sketched: neither the cares of his great Catalogue, nor the labours which the discovery of Ceres had required, nor even a fever which under-

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mined his health for four years, could for a moment divert him from his studies. The positions assigned by Maskelyne to several stars were almost immediately mistrusted; but Piazzi was too much engaged in his researches to think of correcting the works of others. He deputed M. Cacciatore, the most distinguished of his pupils, to compare directly the principal stars with the sun.

This work was not confined to the thirty-six stars of Maskelyne; it contained one hundred and twenty, which served as the basis of the new Catalogue. Piazzi did not finish it until 1814, and it was not without astonishment that he was found to have extended his researches to 7646 stars. Urged by his friends and his pupils, Piazzi occupied himself with preparing several memoirs which he intended for several Academies of which he was a member: he held at the same time some commissions which the government of Naples had given him; among others, the formation of a metrical code, to establish a uniformity of weights and measures in Sicily. His work was preceded by an Essay, published in 1808, and by Instructions intended for the use of the *curés*. During the constitutional government of the kingdom (in 1812), Piazzi was consulted upon a new territorial division, which was decreed by the parliament, according to the report of the astronomers, and has been preserved even since the destruction of the representative government. The comet of 1811 gave Piazzi an opportunity of explaining his ideas upon the nature of these bodies. He did not suppose their formation to be contemporaneous with that of the planets: he was rather of opinion that they were occasionally formed in the immensity of space, in which they are afterwards dissipated, nearly like those globes and luminous meteors which are generated and disappear in the terrestrial atmosphere. With such opinions, it is not surprising that he always attached but little importance to the observing of comets.

In 1817, Piazzi was called to Naples to examine the plans of the new observatory, founded by Murat, upon the heights of Capo-di-Monte. He introduced many changes, of which he gave an account in a work published a little before his return to Palermo. Succeeded in the immediate direction of this observatory by his pupil Cacciatore, he took an active part in the labour of a commission charged with the public instruction in Sicily, a country which he regarded as a second home, and which he preferred to the brilliant offers made to him by Bonaparte, to draw him to the University of Bologna.

Piazzi had no less constancy in his affections, than perseverance in his studies: he had collected an uninterrupted series

of

of solstitial observations, from 1791 to 1816, to determine the obliquity of the ecliptic. On comparing them with those which were executed in 1750, by Bradley, Mayer, and Lacaille, it will be observed that the obliquity undergoes a diminution of 44″ in every century.

The last arrangements of this great astronomer furnished fresh proofs of his love for science. He bequeathed his library and his apparatus to the observatory of Palermo, adding an annual sum for the education of a pupil. Piazzi enjoyed a just reputation, acquired by his innumerable and important labours. He was director-general of the observatories of Naples and Palermo, president of the Academy of Sciences of Naples, member of that of Turin, Gottingen, Berlin, and Petersburg, foreign associate of the French Institute, of the Royal Society of London; ordinary member of the Italian Society, and corresponding member of the Institute of Milan, &c.

XXXV. *Continuation of the Subject relating to the Absorption and Extrication of Heat in a Mass of Air that changes its Volume.* *By* J. Ivory, *Esq. M.A. F.R.S.**

In treating the subject of this article in the last Number of this Journal, care has been taken to avoid all assumptions merely hypothetical, and to ground the reasoning on acknowledged facts. My chief purpose at present is to show that the conclusions which have been obtained lead to a very direct and simple solution of the important problem concerning the velocity of sound in the atmosphere, which has hitherto been investigated in a manner rather complicated and circuitous. I shall also be able to correct some inaccuracies that have crept into the mathematical part of this research, and which have arisen from an obscure and imperfect knowledge of the relations that subsist between the quantities concerned. But before entering on these topics it may not be improper to recapitulate briefly, the main grounds on which the theory proceeds.

In the first place, when heat is applied to a mass of air under a constant pressure, the variations of volume are proportional to the quantities of absolute heat which produce them. The same thing in effect may be enunciated by saying in the usual phrase, that the specific heat of air under a constant pressure is the same at all temperatures. The proposition must not be understood absolutely and indefinitely: we ob-

* Communicated by the Author.

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tain our knowledge of it experimentally, and by experiment it must be limited. MM. Dulong and Petit have compared the expansion of air under a constant pressure with the indications of a mercurial thermometer; and their researches prove that the increase of volume keeps pace with the ascent of the mercury between the points at which mercury freezes and boils, that is, from -40° to about 600° of Fahrenheit's scale*. We must therefore infer, that for this long range of temperature, equal quantities of absolute heat have caused equal increments of volume both of the air and the mercury.

In the second place, when heat is applied to air which is kept from changing its volume, the elasticity increases at the same rate with the temperature. There is here no absorption or disappearance of any part of the heat; the whole of it is employed in augmenting the elasticity. But although the effect is single, yet, because the condition of the air varies as more heat is applied, we cannot infer that equal increments of elasticity accompany equal rises of temperature. This is a point that experience must determine; and, from the researches of MM. Dulong and Petit and other natural philosophers, it appears, that the increase of elasticity keeps pace with the rise of temperature within the limits already mentioned, that is, from -40° to 600° of Fahrenheit's scale†.

It is next to be shown that when air under a constant pressure expands by heat, the whole heat it acquires is resolvable into two distinct and independent parts; the *latent heat* which unites with the air as the volume increases without affecting the thermometer, and the *heat of temperature* which is capable of raising the temperature from the initial to the ultimate quantity, the volume remaining invariable. In order to prove this, it is to be observed, that a given mass of air may be made to change its bulk and temperature in two different ways. First, the pressure remaining invariable, the air may be dilated to any proposed volume by the direct agency of heat. Secondly, the same mass of air may be allowed to expand to the same volume without the application of heat, either by enlarging the dimensions of the containing vessel, or by lessening the pressure; and when this is done, the volume being kept from changing, the temperature is next to be raised to the same degree as in the first process. In this second method of operating, heat enters the air as it expands; and as the temperature is the same both before and after the expansion, it follows that the *latent heat* depends solely upon the increase of volume, and is perfectly distinct from the heat of tempera-

* *Journal de l'Ecole Polytech.* tom. ii. p. 200.

† Ibid. pp. 199, 200.

ture

ture afterwards communicated. Now by both processes the air is ultimately brought to the same condition, and consequently it must have acquired the same quantity of heat. It is therefore proved that the whole heat acquired by air which expands under a constant pressure, is composed of two independent parts, namely, the *latent heat* and the *heat of temperature.*

Let us next compare the dilatation of a mass of air under a constant pressure, or, which is the same thing, an air-thermometer, with the indications of a mercurial thermometer: the whole heat acquired by the air will be proportional to the ascent of the mercury or to the increase of the bulk of the air: and again, according to what is shown above, the heat of temperature will likewise be proportional to the ascent of the mercury, or to the increase of the bulk of the air; wherefore the difference of these two heats, that is, the latent heat, must be proportional to the ascent of the mercury, or to the increase of the bulk of the air. It thus appears that the three heats, namely, the whole heat acquired by the air and its two parts, the heat of temperature, and the latent heat, receive, each, equal additions for equal increments of the bulk of the air; and consequently for any given dilatation, they will always bear the same constant proportions to one another. And this must be admitted as true for a very extensive range of temperature, or so long as thermometers of air and mercury continue to measure heat exactly.

When air expands under a constant pressure, a rise of one degree of Fahrenheit's thermometer has been found to correspond to an increase of volume equal to $\frac{1}{480}$th of the bulk possessed by the air at the freezing of water. It follows, therefore, that, in order to know the latent heat absorbed in any dilatation, or disengaged in any condensation, we have only to investigate the invariable proportion it bears to the heat of temperature capable of producing the same change of volume under a constant pressure. Now this invariable proportion has been deduced in the last Number of this Journal, from an experiment of MM. Clement and Desormes, and it comes out equal to $\frac{3}{8}$ nearly. Such is the nature of the experiment mentioned, that it leads to a proportion rather below the truth; but we may correct the result by the velocity of sound in the atmosphere, which agrees better with $\frac{2}{5}$ than $\frac{3}{8}$. Hence it appears that, when air expands under a constant pressure, the whole heat it acquires for any increase of volume, the heat of temperature and the latent heat, are as the numbers 11, 8, 3, or, more nearly, as 7, 5, 2. If we apply to a thermometer of Fahrenheit's construction a scale having the distance between the

the freezing and boiling points divided into about 70 equal parts or degrees, the rise or fall of the mercury on this scale will show the latent heat of a mass of air varying its volume under a constant pressure, at the same time that the usual scale marks the temperature.

In what goes before, our attention has been occupied exclusively with atmospheric air; but it will readily appear that the conclusions obtained extend to all the gases. For it may be shown by the same reasoning as in the case of air, that when a gas expands under a constant pressure, the whole heat it acquires, is resolvable into latent heat and heat of temperature; and that these parts are distinct from, and independent of, one another. It is also a principle that holds good as far as our experiments enable us to judge, that, for equal rises of temperature, all the gases expand at the same rate as air. If now we compare two thermometers, one of air and one of a gas, the whole heat acquired by each fluid, in any given dilatation, will be the same; the heats of temperature will likewise be the same: consequently the latent heats must also be the same. We are therefore to conclude, that when a gas expands under a constant pressure, the whole heat acquired in any dilatation, the heat of temperature, and the latent heat, are to one another as 11, 8, 3, or probably more nearly, as 7, 5, 2.

The theory which we have been explaining suggests some reflections concerning the agency of heat. When it expands air or a gas, it raises the temperature, and it enlarges the volume without affecting the thermometer. These effects are independent of one another; for they may be exhibited separately, and either of them may be carried to any extent while the other remains unchanged. The latent heat enters the air and unites with it in a manner not perceptible to our senses, and increases the bulk: the heat of temperature augments the elasticity and affects our senses. Does heat operate according to these laws only in the case of air and the gases? or, rather, is it not our power over the pressure, by which we can dilate or contract a given mass of elastic fluid as we please, that has enabled us to investigate the effects in question? When heat is applied to a solid or a fluid, its expansive force acts against the cohesion, over which we have no control. We cannot expand either of these kinds of body, and at the same time keep the temperature constant; neither can we raise the temperature, and at the same time keep the bulk unchanged. The mode of investigation that has been pursued in air and the gases, becomes impossible in solid and fluid bodies; but this does not prove that heat may not operate exactly alike in both

both cases. In favour of the inference that its mode of acting is similar, we have at least a strong argument from analogy. It is proved that the heat which dilates air or a gas, spends its whole force in producing this single effect, and is concealed from the thermometer:—why should it not follow the same law when it expands a mass of iron, or a portion of water or mercury? There seems to be no kind of difference between the two cases, except that, in one, the experimental proof is at hand, and, in the other, it is placed beyond our reach. But in the continued application of heat to solid and fluid bodies, there are two memorable stages at which we are enabled to contemplate the mode in which it operates, while the temperature remains constant, and while it rises without the afflux of extraneous heat. These occur in particular relations between the expansive force of heat and the cohesion; when the former overcomes the latter, and when it is overcome by it. The melting of a solid body, and the conversion of a fluid into vapour, are instances of the power of heat overcoming the cohesive force; and, during all the time the changes are going on, the temperature remains constant; the whole supply of extraneous heat being absorbed and employed in expanding the new fluid or vapour. The reverse processes of a fluid passing into a solid, and of a vapour condensing into a fluid, are instances of the power of heat being overcome by the cohesive force; and here the extrication of heat before concealed, causes a rise of the thermometer till the transformation of the bodies is completed. The first instances are similar to the absorption of heat which always accompanies the enlargement of the volume of an elastic fluid; the second resemble the evolution of heat when the fluid contracts its bulk. By the remarkable phænomena we have mentioned, which were first accurately examined and explained by Dr. Black, the argument for the generality of the law relating to latent heat is much strengthened.

We employ the terms latent heat and sensible or free heat, not in reference to any hypothesis concerning the nature of that power, but to denote effects actually observed when it acts upon matter. Latent heat is that which expands bodies, which produces this single effect and no other, remaining concealed from the thermometer. Heat of temperature, or free heat, on the contrary, affects our senses, and is ready to diffuse itself around whenever the equilibrium is broken. In the two cases, if it be allowed that the facts are equally general, the phraseology must be alike unexceptionable. The modes of speaking relate entirely to modes of acting. Heat

in combining with matter never changes its nature; it is never annihilated; it passes from free heat to latent heat, and the contrary, according to circumstances. The only question is about the generality of the fact; whether it be true that heat which expands bodies is always concealed from the thermometer. We have proved that it is true in elastic fluids; and analogy, aided by the discoveries of Dr. Black, affords a strong argument that it holds without exception.

The theory we have been explaining is nowise inconsistent with the doctrine of specific heat and capacity. We have here compared the quantities of heat which unite with bodies when their temperature is raised, with the dilatation which they produce. But we may likewise compare with one another the quantities of heat requisite to cause a given rise of temperature in different bodies; and, in this view, they are called specific heats, and the bodies themselves are said to have different capacities for heat. These two ways of considering the manner in which heat combines with bodies, are clearly distinguished. The one by no means supersedes the other. On the contrary, we may deduce from the property of latent heat we have endeavoured to establish, the condition which causes the capacity of a body to be constant, or to vary. Whenever equal additions of latent heat produce equal increments of volume, the capacity must be constant; otherwise it must vary. This will readily appear, if it be considered that it is the latent heat which causes the expansion, and that we employ the expansion to measure the free heat, or the temperature. The specific heat of bodies is, therefore, plainly regulated by the latent heat. But in other respects the doctrine of capacity leads to considerations of which we have had no occasion to speak.

The observations I have been led to make have carried me far beyond my original intention, and I must reserve what further remains on this subject for a future occasion.

Feb. 5, 1827. J. Ivory.

XXXVI. *Notice relating to the Seconds Pendulum at Port Bowen.* By J. Ivory, *Esq. A.A. F.R.S.**

THE 4th part of the Philosophical Transactions just published, contains an experimental determination of the seconds pendulum at Port Bowen, a station in Prince Regent's Inlet, by Lieutenant Henry Foster, R.N. F.R.S. The result

* Communicated by the Author.

of

of this experiment and the comparison of it with my formula, Phil. Mag. for Oct. 1826, are as follows:

Latitude.	Observed pendulum.	Computed pendulum.	Excess of calculation.
73° 13′ 39″·4	39·20347	39·20265	—·00082

The error is not great: and this is the 29th experiment represented by my formula with small discrepancies.

The latitude of Port Bowen being little more than a degree short of that of Captain Sabine's station at Greenland, we may compare the two experiments.

	Latitude.	Observed pendulum.
Sabine	74° 32′ 19″	39·20335
Foster	73 13 39	39·20347

Here the pendulum has shortened for an increase of latitude equal to 1° 18′ 40″. But it ought to have lengthened at least ·00250. Thus there is a discrepancy between the experiments of the two observers, greater than between my formula and Captain Sabine's result.

Port Bowen is in the middle of Captain Sabine's northern stations. We may therefore employ Mr. Foster's experiment to compute the pendulums at those stations in different hypotheses of ellipticity, in order to compare them with the experimental determinations of Captain Sabine. Put l and λ for the length of the pendulum and the latitude, at Port Bowen; and let l' and λ' denote the same things for any of Captain Sabine's stations; then,

$$l' = l - f(\sin^2\lambda - \sin^2\lambda').$$

According to my formula, $f = 0·20835$; and according to Captain Sabine's calculations, $f = 0·20227$; and these values may be considered as nearly the greatest and least that can be assigned with any probability. Calculating, now, with these data, we get:

Station.	Observed pendulum.	Computed pendulum $f = 0·20835$	Computed pendulum $f = 0·20227$
Drontheim . .	39·17456	39·17920	39·17990
Hammerfest .	39·19519	39·19804	39·19820
Greenland . .	39·20335	39·20605	39·20597
Spitzbergen .	39·21469	39·21436	39·21404

The computed quantities are very consistent with my formula; but they do not agree well with the observed pendulums. In particular the discrepancy at Drontheim, computed from Mr. Foster's experiment on one side, is nearly equal to

what

what it was before found to be by calculating from Unst and Stockholm on the other side*.

I confine myself to these observations which must stand as long as any trust can be put in the rules of arithmetic. To venture upon any discussion concerning the cause of the singular discordance between Captain Sabine's experiments and those made by other observers, might possibly stir up an altercation of no pleasant kind.

Feb. 5, 1827. J. IVORY.

XXXVII. *An Account of M.* Longchamp's *Theory of Nitrification; with an Extension of it. By* THOMAS GRAHAM, *M.A.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

M. LONGCHAMP, in a memoir read some time ago before the Academy of Sciences, and published lately in the *Annales de Chimie et de Physique*, (t. xxxiii. p. 1.) has developed a theory of the natural production of nitre in various soils, and superficially upon certain rocks. This theory, in its full detail, is, perhaps, not altogether new; for several of the opinions of which it consists have been advocated, or at least broached, by preceding chemists. But M. Longchamp has certainly the merit of confidently displaying these opinions in their full force, and of methodizing them into a consistent system. Of this theory we propose to give an account, as nearly as possible in the words of the author, and to subjoin certain speculations, with the view of supplying a material deficiency in the theory of M. Longchamp.

It may be premised that M. Longchamp confines himself to the production of the acid of the native nitrous salts, and very properly avoids any supposition of the production of their base, previously existing as fact and reason point out that it must be, and, unlike the nitric acid of these salts, incapable of a synthetic formation.

There is reason to doubt the original proposition of Glauber, and which as far as regards the nitric acid has been the prevailing theory to the present day, that "saltpetre is formed by the decomposition of animal and vegetable substances;" for nitrates form and are found in materials and in places which contain no vegetable or animal matter, and which have never been exposed to the emanations of animals.

* Phil. Mag. Oct. 1826, p. 251.

Persons

Persons engaged in the production of nitre know well, that earths taken from caves furnish nitrates by lixiviation, and that earths, replaced in the same circumstances, yield again, after eight or ten years, new quantities of saltpetre. This fact cannot be denied; but some have attempted to weaken its force, by the reflection, that in general the nitrous materials are not completely deprived of their salts by the washing to which they are subjected; while these materials, exposed again to the air, become dry, and as the water does not evaporate except at their surfaces, it deposits there all the nitre which it held in solution. This objection would be of weight, if it were true, that only a small quantity of nitre could be obtained from materials which had been replaced; but it is well known that if earth from a cave has given by the first lixiviation 100 parts of nitric acid saturated with the different bases, the whole mass being returned to the same place, will yield again, after eight or ten years, the nitrates which represent the same quantity of acid. It is not, therefore, only the nitre which the materials have retained, which is obtained by the second lixiviation; but besides, and for the greater part, what is formed anew upon replacing the earth in the circumstances which had induced its first nitrification. Moreover, the same materials twice lixiviated, returned again to the same cave, will yield, after eight or ten years, the same quantity of nitre which they furnished at each of the two former lixiviations; and the nitrification is perpetuated without a limit, provided that the returned earth possess a sufficient portion of the base, which commonly solicits the formation of the nitric acid, and absorbs that acid as it is produced.

Lavoisier took from the quarry a great number of specimens of chalk, at Roche Guyon and Mousseaux, and all when washed yielded a small quantity of nitrate of potash, mixed with much nitrate of lime. These specimens were frequently taken at a distance of many hundred toises from any habitation, and from parts of the rock exposed to the rain and all vicissitudes of weather; and he has drawn this consequence from the facts related in his memoir: "the nitric acid does not pre-exist in the chalk of Roche Guyon, but is formed by the action of the air*." It is remarkable that this chalk was often richer in nitre than the best nitrous soils. The quantity of nitre, which any specimen contained, was found to depend most upon its vicinity to the surface. As the organic remains of these rocks do not retain their animal matter, no in-

* *Mémoires Etrangères de l'Academie des Sciences,* xi. P. II. pag. 565.

fluence

fluence can be attributed here to the decomposition and putrefaction of animal substances in contact with the air.

But nitric acid forms in the open air, and in materials which contain no vestige of animal or vegetable matter. An experiment is related by one of the competitors for the French prize *, in which a quantity of earth from the fields, washed with great care, dried by exposure to the sun, and afterwards kept moist by occasional watering for a year, afforded by lixiviation a saline solution, in one case of one degree of the areometer, and in another of half a degree. Thouvenel, too, who has produced nitric acid by exposing chalk to the gases evolved from the putrefaction of animal or vegetable substances, mixed with common air, likewise obtained this acid when the chalk was in contact with nothing but atmospheric air †. It is true that in the experiment which he relates, the materials exposed to the atmospheric air loaded with putrid gases, yielded fifteen parts of nitrate of lime; while those which were in contact with pure atmospheric air, afforded no more than six parts of the salt. Thouvenel concludes, "It is demonstrated by our experiments, that atmospheric air possesses all that is necessary to serve for nitrification, as well as the air which emanates from putrescent bodies, provided it finds matter capable of absorbing the materials ‡."

M. Longchamp having thus shown how ill-founded the proposition is, that the materials proper for nitrifying *never* nitrify in the air, without the concurrence of animal matter, attempts, in the next place, to prove that the nitric acid is formed exclusively from the elements of the atmosphere.

It is admitted, he observes, that the animal matters do not require to be in contact with the earths, but that their emanations are sufficient for the production of nitre. Could it be through the instrumentality of azote, which animal matter might disengage during putrefaction? But chemists know that the products of this putrefaction are ammonia, carbonic acid, carburetted hydrogen, and perhaps some carbonic oxide and water, but no azote; and even if this gas were produced, how would it combine with the carbonate of lime? There are instances of extraordinary combinations of gases in the nascent state, but the azote is not presented in that state in the case referred to, since the putrescent blood was at the distance of two feet from the carbonate of lime, which it is pretended that it nitrified §. Might

* *Mémoires Etrangères de l'Academie des Sciences*, xi. P. I. pag. 160.

† *Ibid.* P. II. pag. 124. ‡ *Ibid.* pag. 89.

§ The commissioners of the Academy, among whom was Lavoisier, took a quantity

Might it arise from some combination of azote, which these emanations bore along with them? But it is known that in the putrefaction of blood, urine, and similar matter, all the azote goes to form ammonia: admitting, however, that a part of the azote escapes the hydrogen, and enters into some combination hitherto unobserved; Why, it may be asked, does it exhibit no nitrifying power without the cooperation of carbonate of lime? For if directed against caustic lime, magnesia, alumina, &c., no nitric acid is formed, or at least a scarcely sensible quantity, and only after a long lapse of time; while if potash, caustic or carbonated be presented, not an atom of nitre is formed*.

Might it be through a reaction of the putrid emanations upon the atmosphere? But, besides that this reaction is difficult to conceive, and that otherwise it would be the azote of the air which formed the nitric acid, and not that of the animal matters, it may still be asked, Why is the carbonate of lime the only body which solicits this reaction?

Considering it as proved, that animal substances do not nitrify by means of their *emanations*, M. Longchamp believes that insuperable difficulties attend the supposition, that putrescent bodies, *in contact* with carbonate of lime, contribute in any measure to the production of nitric acid. For there is no chemical fact which entitles us to suppose, that urine or blood would yield by their putrefaction, other products when they are mixed with calcareous earths, than when they putrefy without the admixture. Provided, too, that the animal matters remained in the solid state, their action upon the solid calcareous matter would be very much circumscribed, extending only to the particles in immediate contact with their surfaces. Even supposing that the animal matter was liquid, and would thereby become diffused more generally through the mass, still its action would be limited to a great degree, by the total insolubility of the carbonate of lime. From a review of these circumstances, Mons. L. considers himself entitled to conclude, that animal matters, whether solid or liquid, do not concur by their azote to the formation of the nitric acid. He then proceeds to the development of his own theory, or to show how atmospheric air, without the concurrence of any vegetable or animal matter, may form nitric acid.

a quantity of the carbonate of lime, which they carefully washed in boiling water to extract all the salts; they placed the washed carbonate of lime in baskets, which were hung at the distance of two feet from a quantity of blood in a state of putrefaction. *Mém. Etrang. de l'Acad.* xi. P. I. p. 126.

* Thouvenel, *Ibid.* P. II. pag. 119.

It

It is universally admitted that nitric acid is not formed in sheltered situations, unless a certain degree of humidity prevails, and the air circulates through all the parts; for in places where the air cannot be renewed, there is no formation of acid. Thus, Lavoisier observed at Roche Guyon, that in the caverns or pits which were very deep and had but one issue, nitric acid did not appear in the deep parts, but only at the entrance. The same observation was made by that celebrated philosopher in the tufa quarries of Touraine. The nitric acid is formed only in places which contain porous rocks or light soils, possessing carbonate of lime, moisture, and a constant circulation of air.

Tufa, light earths and chalk, act chiefly as absorbents. Chevraud met with compact chalks which did not nitrify. Hence we never find marble, whether in the quarry exposed to the atmosphere, or in our houses, to exhibit any tendency to the formation of nitre; while tufa and chalk, which differ from it only in porosity, nitrify with ease.

It is upon water that chalk and tufas exert their absorbing power. But these substances in contact with water, produce no nitric acid when atmospheric air is withheld. But the water brings air with it, and the nitrifiable materials, possessed of humidity, continue to absorb air by means of that humidity.

Chemists have long known that all kinds of water contain air; but to MM. Gay-Lussac and Humboldt * we are indebted for a fact, which has more recently been confirmed by the latter philosopher and M. Provençal †, that the air in water contains more oxygen than atmospheric air does. The mean of ten experiments made by Humboldt and Provençal on air derived from water, gives the proportion of oxygen as $\frac{3105}{10000}$. The previous researches of Gay-Lussac and Humboldt made us acquainted with a still more interesting fact, that if aërated water be exposed to heat, and if we divide the air procured into any number of equal portions, the first portions contain less oxygen than the last, as is exhibited in the following table:

Oxygen in 1000 parts of	1st portion of air	24·0
	2d	26·8
	3d	29·6
	4th	33·0
	5th	34·8

M. Longchamp's application of this fact I shall give in his own words, without abridgement,—the more so, as I consider it not altogether correct. "According to M. Berzelius, prot-

* *Journ. de Phys.* lx. 129.

† *Mém. d'Arcueil.* ii. 359.

oxide

oxide of azote contains 36·07 parts oxygen; the last portion, therefore, of the air obtained in the experiments of Gay-Lussac and Humboldt, contained almost as much oxygen as the oxide of azote possesses: and we perceive that water exercises such an action upon the oxygen and azote, as tends to combine these gases in a more intimate manner than they exist in the atmosphere. But if any other force should unite with that of the water, is it not reasonable to think that the molecular action of the gases will acquire more energy, and that there will result from these united forces a combination which will be nitric acid; whether this acid is formed in following out the whole chain of compounds known and unknown of oxygen and azote, or is formed immediately by the first action of these gases? Now, the body which in nitrification seconds the action of the water, is the lime of the chalk. So then, tufa, chalk and nitrifiable materials act in nitrification both as absorbents of water and air, and as presenting a base which solicits the formation of nitric acid; and water acts as an absorbent of oxygen and azote, and in commencing the combination of these gases."

The greater portion of oxygen absorbed depends without doubt simply upon the greater absorbability of that gas than of azotic gas, and not as Mons. L. supposes, upon water exerting "such an action upon the azote and oxygen as tends to unite them in a more intimate manner than they exist in the atmosphere." We embrace, however, M. Longchamp's fundamental proposition,—that it is from the action of the oxygen and azote, held in solution by water, upon the carbonate of lime, that the nitrate of lime results. All bodies, when in the liquid state, possess their powers of combination most energetically. Now I have formerly shown* that oxygen and azotic gases, when absorbed by water, are really in the liquid state; there is, therefore, some reason for that activity with which our theorist has invested them.

Such is the theory of M. Longchamp; and it appears to me to be, as far as it goes, a true explanation of the phænomena. The process of nitrification is constantly going on in nature, and in circumstances where no other agents appear to be employed, except carbonate of lime and the elements of the atmosphere. Hence, in circumstances in which animal matter is superadded to these agents it is reasonable to think that the latter does not contribute, in any essential way, to the nitrification. Where nitrate of potash is the ultimate result, it appears to be established that nitrate of lime pre-existed, and

* Annals of Philosophy, N. S. vol. xii. p. 69.

that the nitrate of potash resulted from the decomposition of the nitrate of lime by some salt of potash.

But it cannot be denied, that the nitrification of calcareous substances is greatly promoted by the contact, or, more generally, by the proximity of putrescent vegetable and animal matter. The experiment of Thouvenel, to which M. Longchamp refers above, abundantly proves this; and the constant and universal practice in the formation of artificial nitre-beds strongly confirms it. This fact appears, therefore, to weigh heavily against the theory of M. Longchamp: it is, however, in our opinion, susceptible of an explanation without any mutilation of that theory; and to this extension of the hypothesis we now proceed.

We are disposed to attribute the beneficial effect in nitrification of the decomposition of animal and vegetable matter, to the plentiful supply of an element which exists at all times in the atmosphere in a perceptible proportion—carbonic acid gas. *The free carbonic acid renders a portion of the carbonate of lime soluble in the water or moisture, which must be present; and thereby enables the carbonate of lime to act more effectually upon the oxygen and azote, which the water has absorbed.* The oxygen, azote and carbonate of lime are all liquefied, and in solution in the water; they are therefore in circumstances most favourable to their mutual action.

Carbonate of lime is altogether insoluble in pure water, while water saturated with carbonic acid dissolves 1-1500th part. According to Dr. Thomson*: "when carbonate of lime is rendered soluble in water by means of carbonic acid, a bicarbonate is formed, which seems only capable of existing in solution." That carbonic acid is one of the most considerable products of the putrefaction of both animal and vegetable substances, is well known.

Water in ordinary circumstances absorbs rather more than an equal volume of carbonic acid gas.

Now Thouvenel, without any view to this point, performed and has registered a series of experiments, which render it exceedingly probable, that of the products of putrefaction, it is the carbonic acid alone which contributes to the nitrification; inasmuch as when these products were deprived of their carbonic acid, by being passed through caustic potash or lime-water, before acting upon the chalk, their nitrifying power was lost; while otherwise their nitrifying power was sufficiently notable. I shall give Thouvenel's experiments as reported by Messrs. Aikin in their Chemical Dictionary, which is

* First Principles, ii. 296.

still

still the best work we possess upon the chemical manufactures.

"Having charged a retort with putrefying materials, Thouvenel connected with it three receivers in the manner of Woulfe's bottles, the last of which terminated in a tube communicating with a pneumatic apparatus. Four different sets of this apparatus were employed at the same time. In the first of these the two receivers nearest the retort were charged with four ounces of chalk diffused in distilled water, while the third receiver contained a solution of caustic potash. In the second set the two first receivers contained distilled water, and the last was charged with washed chalk. In the third set the two first receivers contained lime-water: and in the fourth set a solution of caustic potash; the third receiver in both cases holding the chalk. They were all equally exposed to the same temperature, namely, from 74° to 80° Fahr., for six months, and the changes which their contents had undergone were then examined.

"The chalk in the first apparatus afforded 26 grains of nitrate of lime mixed with a little nitrate of ammonia; the potash in the third receiver had become saturated with carbonic acid, and had partly crystallized on the side of the receiver, but contained no nitre.

"In the second apparatus the water of the two first receivers had acquired a very putrid smell from the gas which had passed through it, and contained a little ammonia, but afforded no nitrous salt on evaporation: the chalk in the third receiver afforded by lixiviation no more than 4 grains of nitrated lime.

"In the third apparatus the lime-water had deposited its earth in the state of carbonate, and the supernatant fluid had a strong odour resembling ammonia and putrid garlic: by evaporation it yielded 5 or 6 grains of nitrated ammonia. The chalk in the third receiver gave only a slight trace of nitrate of lime.

"In the fourth apparatus the potash was crystallized, but contained no nitre: with sulphuric acid it effervesced strongly, giving out a very pungent and highly fetid gas: *the chalk in the third receiver gave no indications whatever of the presence of any nitrous salt.*

"The gas remaining in the receivers and collected in the pneumatic apparatus, was in all the four experiments found to be slightly inflammable, although when rising from the putrefying materials it extinguished a taper immersed in it. This putrid inflammable gas was incapable by itself of nitrifying chalk; but when mixed with washed atmospheric air, *carbonic*

acid soon made its appearance, and then the gas became capable of impregnating chalk with nitrous acid as at first *."

These experiments of Thouvenel, and particularly the last observation, point out carbonic acid as the important agent in nitrification, at least as distinctly as could be expected of experiments of this nature.

It has all along been observed in the management of artificial nitre-beds, that although free exposure to the atmosphere be indispensable to the progress of nitrification, yet a strong current of air is exceedingly prejudicial. The rapid circulation of the atmosphere would be attended with the quick dissipation of the carbonic acid gas, upon which we have supposed the superiority of these nitre-beds to depend.

The atmosphere at all times and places abounds in carbonic acid gas, as the exposure of lime-water would quickly indicate. In those chalks and calcareous soils, in which the spontaneous production of nitrous salts is observed, the activity of the carbonate of lime may, therefore, equally depend upon its *dissolution*, effected by the absorption of moisture and carbonic acid from the atmosphere. It would still, however, be a curious subject of inquiry—whether these soils and chalks do not, in some cases, contain within themselves the carbonic acid necessary in conjunction with water to effect their partial solution, and be thus enabled to act to a greater extent upon the absorbed oxygen and azote—the elements of nitric acid?

Should this theory of the instrumentality of carbonic acid, in nitrification, be eventually substantiated, several improvements, in the artificial production of nitre, might evidently be deduced from it.

XXXVIII. *A Sketch of the Natural Affinities of the* Lepidoptera Diurna *of Latreille. By* William Swainson, *Esq. F.R.S. F.L.S. &c.*†

To those who have traced the progress of human knowledge, or are themselves engaged in its pursuit, it must appear evident that its extent would have been much greater than it really is, were we not so frequently withheld from communicating that which *we know*, from a sense of the importance of that which we do *not know*. Hence it is, that undertakings long meditated upon, and even carried beyond the point to which others have reached, are frequently laid aside on the appearance of some unexpected difficulty or temporary embarrassment.

* Aikins' Chemical Dictionary, vol. ii. 160. From *Mém. Etrang. de l'Acad. des Sciences*, tom. xi. 503.

† Communicated by the Author.

barrassment. This, it is true, in progress of time may frequently be overcome; but no sooner do we begin to make a little progress, than other doubts arise, which can only be solved by information which we again wait for. Thus months and years pass away, and that knowledge which, if properly used, might have advanced others one step nearer to the Temple of Truth, is suffered to lie useless and unemployed.

I have been led to these reflections, by having lately had occasion to bring together all I can find hitherto written on the *Lepidoptera*, and to revise what I had myself done on the same subject, some years ago. In the winter of 1823 I attentively studied these insects, with a view to discover their natural affinities; and I communicated the result to several of my entomological friends in the following spring. I deferred however the publication of these views, at the time, from a desire of procuring further information upon several points, then involved in obscurity. These have long since been cleared up; but other difficulties presented themselves; and it is probable that but for the necessity I am now under of introducing this subject in a larger work, the essay of which the following is a sketch would still have remained neglected.

Before entering upon this subject, it may be as well for me to express my firm conviction that the Almighty Author of the universe has created all things that have life upon one plan; and "that this plan is founded on the principle of series of affinities returning into themselves *;" which can only be represented by circles. This sublime discovery, sufficient of itself to immortalize a name, was first made known to the world by our illustrious countryman. It was soon after confirmed by two other eminent philosophers †, unknown to each other, and finally has been proved to demonstration. Yet the right application of these principles to the race of beings *now existing* upon our globe, is another consideration; on which there is, and always must be, great diversity of opinion. The temple has been shaken, and in part destroyed; and although a sufficient portion remains to give us some faint idea of the original beauty and perfect harmony it once exhibited, the restoration of the fragments will long continue to engage the speculations and inquiries of the beholder. One fact, however, is certain, That where we find the series of any particular group unbroken by sudden or abrupt transitions, it will always be found to contain five others of an inferior description, two of which will exhibit a perfection superior to the other three. And it is no less certain, that this law of Nature is most con-

* MacLeay, *Horæ Entomolog.* Part ii. p. 459.

† MM. Fries and Decandolle. See Linn. Trans. vol. xiv. p. 62.

spicuous

spicuous in those groups where the series of affinities is so perfect, and the change so gradual, as to set at defiance all possibility of separating the minor divisions by absolute and exclusive characters.

Now, upon looking to the *Lepidoptera*, it does not require any prejudice in favour of the foregoing principles, to discover five prominent groups of nearly equal magnitude, which may be represented by the genera *Papilio*, *Sphinx*, *Bombyx*, *Geometra*, and *Noctua* of the Linnæan school. And further, that while the two former are typical, the first represents the greatest perfection, and the second contains not only types of the other four divisions, but nearly so of all the subordinate groups.

In selecting the *Lepidoptera Diurna* of M. Latreille as a subject for the present sketch, I shall avoid entering into details of those reasons which have induced me to abandon the different arrangements proposed by others. Those of Linnæus and Fabricius were confessedly artificial; although the minor groups of the latter deserve to have been better known, and more generally adopted. The first attempt that I can discover towards a natural method, is that published by Geoffroy in 1764*, which in all probability furnished the basis of the classification adopted by the celebrated Latreille. Both, in fact, are founded upon characters drawn from the larva and pupa, and the partial or full development of the anterior feet in the pefect insect. The former are so much diversified, as to lead us to imagine that by attentively studying and judiciously combining their forms, we may obtain some certain clue to thread the labyrinth of affinities; or, at least, that we shall make a nearer approach to the truth, than if we looked only to the shape of the wings, or the nails of the tarsi. Yet it must be confessed that difficulties are opposed to this line of inquiry, which, in the present state of entomological knowledge, seem to me insurmountable. The larvæ of many considerable groups inhabiting distant regions are to this day utterly unknown: and even among those contained in the valuable works of Stoll and Abbot, there exists such a striking diversity in the forms of larvæ belonging to insects of the same natural group, that no certain conclusions can, at present, be made upon the subject.

The pupa state likewise presents many remarkable variations. Yet as, upon the whole, it is confined to much fewer forms, and these forms are better understood, there seems no

* Was this borrowed from the illustrious DeGeer? His invaluable "*Mémoires*," now of very rare occurrence, I unfortunately do not possess.

reason

reason to doubt, that in our present difficulties, more accurate results will be derived by an attention to this state of the insect, than can be expected from a paramount regard to the larva. Entomologists have long ago remarked the following variations in the form and suspension of diurnal pupæ.

1. Pupa suspended by the posterior extremity. 2. Pupa attached by the posterior extremity, but braced or supported in a horizontal or vertical direction by a transverse thread. 3. Pupa attached like the last, but foliculated, or inclosed within a leaf. Among these, two forms are conspicuous:— 1. Pupæ elongated and angular. 2. Pupæ obtuse and smooth.

Geoffroy and Latreille have not failed to draw a marked distinction between such of the diurnal *Lepidoptera* as have the anterior feet perfect, that is, distinctly furnished with claws, and those which have the same feet imperfect, or not furnished with claws.

It is clear that in any attempt towards a natural arrangement, all these variations must have their due weight, yet without being used as circumscribing bounds. There is presumptive evidence to prove the truth of the assertion, "that the variation of metamorphosis (or of any particular set of organs) is only an index of the series of affinity, and not a principle by which groups have been strictly circumscribed*."

On searching for that group which presents the most perfect development of organs, and at the same time is eminently distinct from the other primary divisions of the *Lepidoptera*, our attention is immediately fixed upon the genus *Papilio* of modern authors. In these the larva is cruciform, the pupa angulated and braced, and the perfect insect furnished in both sexes with distinct nails on their anterior feet. These characters, strikingly exemplified in the typical groups (*Papilio*, *Pieris*, L.) are softened down, and in part exchanged for others in the aberrant examples. In the genus *Colias* for instance, the anterior feet are short, and the *ungues* small and weak: in *Thaïs* the club of the antennæ is elongated and arched like that of many *Hesperidæ*; and in *Parnassus* the pupa is smooth and subfoliculated †. To this last genus we shall subsequently have to call the reader's attention.

By the short and weak feet of *Colias*, we are conducted to a large and very important division, comprising many forms defined by Fabricius, but classed by Latreille as the genus *Nymphalis*. If strength of body, rapidity of flight, superiority of size, or brilliancy of colouring, were sufficient to constitute the typical

* *Horæ Ent.* part ii. p. 456.

† Latreille, *Gen. Inst.* See also *Esper.* vol. i. tab. 2. f. 1.

perfections

perfections of the diurnal *Lepidoptera*, the superiority would undoubtedly be conferred upon this group, did they not show a decided inferiority to the last in their imperfect construction. The anterior legs are destitute of claws, and are so short, as to appear at first sight perfectly useless. The angular form of the pupa is still fully preserved; yet, instead of being braced, it is suspended only by the posterior extremity. The lower wings of the perfect insect are dilated, so as to form a groove for the defence and support of the short conic body. It is obvious that this particular construction is admirably adapted for giving to these insects that superiority of flight for which they are remarkable. Yet in this power there are gradations: it is most developed in the genus *Paphia*, F., but is diminished in *Morpho;* the former is conspicuous for strength, the latter for size and beauty. Tracing this gradation further, we find in the genus *Hipparchia* a feeble and irregular flight, and a considerable falling off, in other respects, from the typical characters.

The following observation of the celebrated Latreille deserves particular attention, in this part of our inquiry. "*Papil.* Crameri: *Phlegia*, *Eugenia*, *Calliope*, *Euterpe*, *Diaphana*, *Lenea*, *Nise*, *Melanida*, etc.; Heliconiorum habitus; horum lepidopterorum sedes naturalis incerta; an genus proprium?" No better authority can be brought forward to show that we have now arrived on the confines of another and a very extensive group, typically represented by the genera *Mechanitis* and *Idea* of Fabricius: these, with *Euplæa*, and probably *Acræa* of the same author, are marked by the same debility in their anterior feet, and the same mode of suspension in their pupa state, as the last: the pupa, however, is obtuse, and quite smooth; while the perfect insect, from its delicate construction, betrays a weakness of flight unexampled among the diurnal *Lepidoptera*. This I have myself witnessed. The feeble texture, and horizontally lengthened wings of *Mechanitis* seem, during flight, scarcely sufficient to support its long and clavate body. As we recede from these, the anterior feet in one or two groups, not hitherto characterized, begin to assume a more decided form, as if Nature was about to quit this type for another. Unfortunately, the slight information we possess on the metamorphosis of these insects, leaves me in much ignorance in this part of my inquiry; and I must content myself with noticing the generally weak construction, and striking similitude between several of the *Heliconi* and *Erycinæ* of Latreille, as circumstances strongly in favour of a natural affinity.

The next division comprises groups of the most singular and

and apparently unconnected, forms; yet all agreeing, so far as we yet know, in the following characters. Larva somewhat ōnisciform. Pupa short, contracted, smooth, and braced. The anterior legs, in some genera approaching to the insects we have just quitted, are very short; in succeeding groups they become progressively longer, and finally, in *Polyommatus*, Latr. the six feet, in both sexes, are alike furnished with nails. These nails are, indeed, scarcely perceptible in *Thecla*, but in *Lycena* they become perfectly developed. The forms and habits of this interesting group are no less varied. They are mostly of a small size; some are remarkable for their sombre, others for their brilliant, colouring. Some are feeble, and when at rest extend their wings horizontally; others, of a more robust make, fly with swiftness, and repose with their wings erect. Like the *Acrita* of MacLeay, this group appears, on a hasty glance, to want that symmetry of conformation so observable in the preceding divisions. Yet this impression soon vanishes, and we discover, in this apparently heterogeneous assemblage, that Nature has given symbolical representations of every form which she afterwards adopts to characterize the leading divisions of the whole tribe. The *Papilionidæ*, *Nymphalidæ*, *Heliconidæ*, and *Hesperidæ*, are not only represented, but every minor group and nearly every principal genus, will find its prototype among the *Erycinidæ*.

To lay any particular stress on the close affinity between the aberrant groups of the *Erycinidæ* and the *Hesperidæ* is quite unnecessary. By Fabricius they were at first united in the same genus, and M. Latreille has placed one almost immediately after the other. The only difficulty is, in ascertaining to what family the *Hesperidæ* are united by affinity at the opposite extremity of their own circle. The characters exhibited by the perfect insects have been sufficiently detailed by others, although the minor groups remain, for the most part, undefined. The larva, in every instance we know of, is eruciform; but the pupa, unlike that of any other division, is foliculated, or hid within a leaf, to which it is additionally attached by a transverse thread or brace. Now on looking to all the groups we have here noticed, we find no approximation to this metamorphos, unless it be among the *Papilionidæ*. The pupæ of most *Hesperidæ*, it is true, are smooth; and so far the affinity between them and the *Erycinidæ* is preserved; yet even here the form is elongated; while there are not wanting instances of Hesperian pupæ assuming something the angulated form seen in *Papilio*. On a due consideration, therefore, of all these affinities, coupled with the fact of the pupa of *Parnassus* having been described by all writers as foliculated,

lated, I can come to no other conclusion than that we have at length returned, by a different route, to the point from which we first commenced our inquiry. And that notwithstanding the apparent dissimilarity between *Papilio* and *Hesperia*, they are, in fact, closely united by certain characters, which, under various modifications, preserve an unbroken thread through the whole circle of the diurnal *Lepidoptera*.

Nevertheless it must be observed, that there is an apparent hiatus between the points of these two groups, so far as regards the forms of the perfect insect: or at least, that the transition is not so gradual and progressive as that which can be traced in the other divisions. But this, I apprehend, may originate either from ignorance of the metamorphos of certain insects already known, or of others which still remain undiscovered. I venture to express this opinion, because, so far from discovering any particular fallacy in the mode of investigation here adopted, I find it has conducted me through difficulties, which, but for this guide, appeared at first insurmountable. I see, therefore, no reasonable cause to doubt it will fail, or become no longer of value, in the present instance. The true metamorphos of *Urania Leilus**, when discovered, may probably confirm the views of M. Latreille; and by assigning to this insect an intermediate station between the two groups, render their connexion perfect. Of the metamorphos of *Castnia* we are equally ignorant: but I am more inclined to consider that on this point also, the views of that celebrated entomologist will be found correct; and that *Castnia* will form an aberrant group among the *Sphingides* connecting them with the *Hesperidæ*, by means of *Hesp. Amycus* of Cramer.

It will be easily seen, that in this faint attempt to thread the labyrinth of Nature, many inferior groups have been passed over. To have enumerated all which have been defined by Fabricius and Latreille, and to have characterized many others now before me, would have swelled this paper to a volume. These I shall examine more in detail at a subsequent period. At present, my chief aim has been to fix the reader's attention to the typical or more prominent forms, and to the affinities by which they appear connected. If these are tolerably correct, the minor divisions will easily arrange themselves on one side or the other. It is the perfection of a natural group, that, however extensive, it cannot be broken up, and arbitrary characters assigned to the different portions. On the contrary, each will be dependent and interwoven with the next; and the

* I have so frequently had occasion to notice, when in South America, the inaccuracy of M. Merian's plates, that I am fearful of citing them as an absolute authority.

whole

whole will present that order, beauty, and harmony, which belongs alone to the works of Omnipotence.

Whether in this feeble effort to illustrate such truths I have made some approach to a correct view of Nature, or whether, deceived by the little knowledge we yet possess, I have used it to construct an artificial system, and thereby given another instance of the misapplication of the Quinary principles, is not for me to judge. Mr. Macleay has justly said, "It is easy, indeed, to imagine the prevalence of a number; the difficulty is to prove it. The naturalist, therefore, requires something more than the statement of a number, before he allows either a preconceived opinion, or any analogy not founded on organic structure, to have an influence on his favourite science." (Linn. Trans. vol. xiv. p. 57.) To bring the foregoing observations to this test, I shall endeavour to exhibit, in the following synopsis, the leading characters of the groups at one view.

Lepidoptera Diurna, Latr.

		Families.
1. *Typical Group.* Pupa angulated.	Pupa suspended. Anterior legs imperfect.	1. *Nymphalidæ.*
	Pupa braced and naked.... Anterior legs perfect.	2. *Papilionidæ.*
2. *Aberrant Group.* Pupa smooth.	Pupa braced & foliculated. Anterior legs perfect.	3. *Hesperidæ*, Leach.
	Pupa braced. Larva onisciform. Anterior legs semi-perfect.	4. *Polyommatidæ.*
	Pupa suspended. Anterior legs imperfect.	5. *Heliconidæ.*

By bringing the *Heliconidæ* back to the *Nymphalidæ*, the five groups (which I shall denominate families) will be united every way, and form a circle. The result will be, that all the most perfectly formed insects will be brought together; at the same time that their several distinctions, in other respects, are preserved. The power of flight (which is the distinguishing character of the *Lepidoptera*) is seen to be developed, diminished, and again increased, in a very remarkable manner.—With regard to analogical relations, the following are among the most striking.

Tetrapod. *Nymphalidæ.*	Body short, thick, conic, reposing in a groove formed by the posterior wings. Club of the antennæ, seldom compressed. Thorax very thick. Flight strong, rapid.	Hexapod or sub-Hexapod. *Hesperidæ.*
Heliconidæ.	Body lengthened, slender, clavate, free. Club of the antennæ compressed, generally spatulate. Thorax small. Flight feeble, slow.	*Polyommatidæ.*

Many other analogies may be noticed among the perfect insects; and many, doubtless, from the larvæ of such as are at present known;—but enough has been said on this point.

In conclusion, I should suggest that the secondary groups,

 or

or sub-families, be distinguished by the termination *ina* or *ana*, as *Papilionina*, *Coliana*, *Paphiana*, &c. This rule, so generally adopted in other departments of natural history by modern writers, will at once explain the station occupied by these groups, in relation to those throughout nature.

Tittenhanger Green, near St. Albans,
Feb. 1st, 1827.

XXXVIII. *On the Crystalline Form of the Hyalosiderite.* *By* WILLIAM PHILLIPS, *Esq. F.G.S. &c.**

DR. WALCHNER of Freiburg not long since described a new mineral, under the name of Hyalosiderite, in Schweigger's *Newes Journal*; and a translation of his communication has appeared in the 63rd vol. of the Philosophical Magazine, and also in the first vol. of the Edinburgh Journal of Science.

Having lately obtained a specimen of that extremely curious mineral, affording several nearly perfect crystals, well adapted for the use of the reflective goniometer, I was induced to measure them by means of it, both because the inspection of them raised some doubts of the correctness of Dr. Walchner's determination in attributing to this substance an octohedron as the primary form of its crystals, and also because he has himself observed, that his own determinations of the measurements he has given, "cannot boast of very great accuracy." But it is somewhat singular that Dr. Walchner should believe his determinations will nevertheless "contribute to adjust in some measure the determinations published in Hausmann's *Spec. Cryst. Metall.* relative to the crystals of iron slags formed in various metallurgical processes;" "for," says Dr. W., "we find a corresponding similarity not only in the forms in general, but also in the angles of inclination of the planes:" and he then observes, that the angles given by Hausman "could not but be very imperfect, on account of the small size" of the crystals.

Dr. Walchner has not said by what means he obtained the two measurements on which he has relied for the calculation of all the others given by him: he says, "the inclination of the planes *d* and *d'* (*a*¹ on *c* or *c'* of the following figure) may be determined most exactly, although on account of the smallness of these crystals, even these measurements remain imperfect;" and immediately adds, "the inclination on *d* on *d'* was determined to be 141°, and the inclination on *d'* on *a* (*c* or *c'*

* Communicated by the Author.

on

on P of the annexed figure) amounted to rather more than 130°."

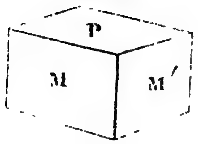
Now I have constantly found by means of the reflective goniometer, that the former of these angles is about 2 degrees less, the latter about 2 degrees greater, than Dr. Walchner's determination. I forbear, nevertheless, from annexing remarks, which naturally arise from a consideration of the preceding extracts from Dr. Walchner's communication; and should have been content with simply noticing the differences between his measurements and those afforded by the reflective goniometer, which alone is adapted for crystals so minute as those of the hyalosiderite (for no one of mine exceeds 1-20th of an inch in any direction), if I could have persuaded myself that some ill-founded prejudice against that admirable instrument, does not exist on the European continent generally, notwithstanding the many errors it has served to correct, the nice differences it has detected, and the ease with which it may be used. Rarely does a foreign mineralogist visit this country, who is acquainted with it except in theory: and I believe that in eve y foreign work on mineralogy, it is figured, not with the moveable pin at right angles, but horizontally, in continuation as it may be termed, with the axis, in which position it is almost useless. These observations are penned in the hope that they may meet the eye of Dr. Walchner, and serve to induce him to prove the superiority of the reflective goniometer, and consequently the futility of the prejudice against it, if in reality it exists.

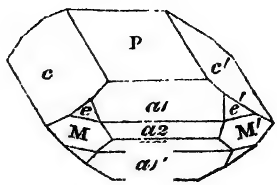
We know, however, that every measurement, by whatsoever means it may be made, is, from the natural imperfection of crystalline planes, rarely to be estimated but as an approximation; but it is also known from experience, that the reflective goniometer is most constant in its results, and the only one adapted for the measurement of small crystals, which commonly are by much the most accurate; for the results obtained by measuring them, agree much better with each other, than those obtained from large crystals.

There is at least one plane on the crystals of hyalosiderite which has not been observed by Dr. Walchner; and which, as it appears to me, is a very important one: it is the plane M of the following figure: most of the crystals in my possession show it, and on some of them it is comparatively large. Its presence has induced me to assume the primary form to be a right rhombic prism of about 105° and 75°. I say, about, because, owing perhaps to the brittleness of these crystals internally, I have not been able to detect with certainty a cleavage in any direction, and because we can only rely upon several

veral coinciding measurements taken upon planes produced by cleavage, which when properly made are usually found to agree,—whilst those taken upon the natural planes generally differ a few minutes. In one instance I found 104° 55′, not 105°. Hence the accuracy of measurements taken as the bases for calculation is very important.

Primary.





	Measurements taken by the reflective goniometer.	Measurements of Dr. Walchner.
M on M′	105° 00′	
P on M or M′ .	90 00	
—— *a* 1	132 32	130° 18′ 56″
—— *a* 2	114 35	119 29 47
—— *c* or *c*′ . .	139 16	141 4 54
—— *e* or *e*′ . .	110 14	
M on *a* 1	124 10	
—— *a* 2	135 5	
—— *c*	113 35	
—— *e*	159 40	
a 1 on *a* 1′ . . .	95 15	99 22 8
—— *a* 2 . . .	162 22	169 10 51
—— *c* or *c*′ . .	120 56	
a 2 on *a* 2′ . . .	130 10	121 0 26
—— *c* or *c*′ . .	108 30	
c on *c*′	98 30	

The two measurements given by Haidinger in his translation of Mohs, vol. iii. p. 111, agree with those of Dr. Walchner.

XL. *The Bakerian Lecture. On the Relations of Electrical and Chemical Changes. By Sir* HUMPHRY DAVY, *Bart. Pres. R.S.*

[Concluded from p. 104.]

VII. *On the accumulation of electricity, and the chemical changes it occasions in voltaic arrangements.*

IN the view of electro-motion adopted by the illustrious inventor of the pile, the metals were considered as the *only* agents

agents which, in proportion to their surface and their number, occasioned the constant circulation of a certain quantity of electricity through the fluids, or the connecting wires in the pile; and the chemical changes occurring in these fluids were considered as mere results, and not necessarily connected with the circulation. The inactivity of combinations where no chemical changes occur, is sufficiently hostile to this view; but an examination of some of the circumstances of the construction of compound electrical combinations, will bring this hypothesis, and that which I have ventured to adopt, more distinctly into comparison.

Let a piece of zinc and a piece of platinum, both in glasses filled with a solution of nitrate of potassa, be connected through the multiplier, and let the glasses be joined by asbestus moistened with the same fluid; the needle will mark electrical action: let the two glasses now be joined by an arc composed of zinc and platinum, in such a manner that the order is Voltaic, *i. e.* that the zinc is opposite to the platinum, in the original combination—the effect will be increased. Now let an arc of pure zinc be introduced; the effect will be less than with the double arc, but superior to that with the asbestus, and the pole of the zinc opposite the platinum will oxidate, and that opposite the zinc will give off hydrogen. Let arcs of other metals be substituted for the zinc; for instance, of tin, of iron, of copper, of silver, of tellurium: the electrical effects will diminish with the oxidability of the metal; and with tellurium, which does not oxidate at the positive pole of a voltaic battery, they will be destroyed; and the case is the same with rhodium, palladium, and platinum.* That the effect does not depend upon any circumstance connected with conducting power is evident; for charcoal, which is a very imperfect conductor, acts like an oxidable metal; and a very fine wire of platinum, terminated by a small piece of oxidable metal, acts more efficiently when the oxidable metal is opposite the negative pole, than if the whole chain had been composed of oxidable metal; but entirely destroys the effect when the oxidable metal is opposite the positive pole.

If the contact of the metals only was necessary for continued electro-motion, these results, in which a simple homogeneous chain is interposed between the fluids, would be impossible; but they are a necessary consequence of the electrochemical theory, in which the destruction of the positive surface by the chemical negative agent is regarded as a necessary condition; and platinum and tellurium acted like zinc, when their surfaces opposite to the platinum were plunged into diluted nitro-muriatic acid.

If

If two, three, or four glasses are used, and two, three, or four arcs of platinum and zinc, the extreme metals of which are connected through the multiplier, a piece of platinum used instead of one of the arcs will not now entirely destroy the electro-motive effect: it will be diminished as if one arc had been removed. The two will act as a single combination; the three as two arcs, and the four as three; and of course in a voltaic combination of 100 arcs, a single piece of platinum substituted for any one of the arcs, will diminish the power of the apparatus only 1-100dth part.

In attempting to protect copper by zinc, in a separate vessel, from the action of sea-water, I found that when the two vessels were connected by moist tow or vegetable substances, or by a wire (even through fine) of any oxidable metal, the protection was complete: but when even a thick wire of platinum was employed, the copper, though in immediate contact with the zinc, became corroded. After the experiment had continued several days, the surface of the platinum opposite to the copper was found tarnished, as if it had been slightly acted upon by the chlorine combined in the sea-water; but this effect had been too feeble to be connected with any sensible degree of electrical polarity in the platinum.

This result, with those mentioned in the preceding pages, seems to show that there can be no accumulation of electricity in voltaic combinations, unless the same or similar conditions of chemical change exist in the elements or single circles composing them; and that under other conditions, the power generated in single circles is either destroyed or diminished according to the opposing nature, or want of conducting power of the chain of intervening bodies. For instance, in the arrangement (mentioned p. 191) of one piece of zinc and one of platinum, the power is doubled by another series of the same kind, destroyed by an arc of platinum, and diminished by an arc of zinc; by a second solution and a second arc of zinc, it is diminished still more; by a third it is nearly, and by a fourth absolutely, *destroyed*.

As the chemical changes always tend to restore the electrical equilibrium destroyed by the contact of the metals with each other in the fluids, it is evident that in cases in which arcs primarily inactive are connected with those primarily active, the chemical changes produced by the electrical attractions must tend to produce in the primarily inactive parts of the combination, an arrangement which must give it a power in direct opposition to that of the primarily active circles; so that when separated, their actions, if any, must be directly the reverse of the other. This result, which I anticipated, I have actually

actually found to be correct; six arcs of platinum in vessels filled with solution of nitre, were connected with a voltaic battery of 50 pairs of plates; of course each arc gave off oxygen, and collected acid round the pole in the place of the zinc, and afforded hydrogen and collected alkali round the pole in the place of the noble metal: on separating the six arcs from the battery, they were found to possess independent action, the poles which were negative being positive, and those positive being negative: in short, the combination acted as if an original one, consisting of acid, alkali, and platinum.

With arcs of zinc, the results were of the same kind, but the electrical effects were much more distinct: as the tarnished zinc in this case added its own negative power to that produced by the contact with the acid.

In trying similar experiments with six arcs of tin, silver, copper, and other metals, and using different saline solutions, it was found that the reversed electrical effects were most powerful with the most oxidable metals, and the most concentrated and most decomposable solutions; and the weakest arrangement of this kind was with arcs of platinum and pure water; yet even in this instance the water had become slightly alkaline at one pole, and acid at the other.

These experiments, showing the nature of the chemical changes in combinations made active by their connexion with voltaic batteries, and the influence of the newly developed chemical agents, fully explain the phænomena of the secondary piles of M. Ritter; and combined with the fact, that the metals are not *perfect* conductors for electricities of very low intensity, they offer a simple and adequate solution of the circumstances observed by M. De La Rive on the interposition of different metallic plates in the fluids connecting together voltaic combinations *.

From the nature of the chemical changes taking place in each single circle of a common voltaic battery, it is evident, that if any small part of a battery for some time in action, is separated from the whole, and made to act as a distinct combination, its powers must be feebler than if it had been originally an independent series; for the electrical action occasioned by the chemical agents developed in it, are such as to counteract the effects produced by the contact of the metals. Whereas, if a small voltaic series is connected with a much larger one, in reverse order, its oxidable in the place of the noble metals, though the whole power of the combination is much weakened by it when in union; yet, when separated,

* *Annales de Chimie et de Physique*, tom. xxviii. p. 190.

it must act with much greater power, as the chemical changes produced are exactly of the kind which must enhance the primary power of the metals. This deduction (a necessary consequence of the electro-chemical theory) I have proved by direct experiment. A series of 6 arcs, composed of zinc and copper and solution of nitre, was connected in the proper order with a voltaic arrangement of 50 pairs, and suffered to remain in connexion for 10 minutes; they were then separated, and made to act as a single battery: their powers were extremely feeble, not certainly one-third as great as those of a combination of the same kind which had been in action (but unconnected) for the same time. Six arcs of copper and zinc were now connected with the same battery of 50, in a reverse or unconformable manner, so that the six plates of zinc gave off hydrogen and attracted alkali, and the plates of copper oxidated and attracted acid. Being separated after a few minutes, and made to act alone, they exhibited powers which appeared three or four times greater than if they had never been in connexion; the zinc resumed a much higher positive, and the copper a higher negative state, than if they had not before been in the antagonist or unconformable conditions.

All these facts bear upon the same point, and confirm the view which I took of the nature of voltaic combinations in the Bakerian Lecture for 1806; in all of which, whether the destruction of the electrical equilibrium is produced by the contact of metals or fluids, it is always restored by chemical changes, and in which the circulation, if it may be so called, depends upon a union of these causes, the direction of the currents being always opposite in the metallic and fluid parts of the combination, so as to produce what may be regarded as an electrical circle.

VIII. *General observations and practical applications.*

To explain the manner in which different chemical agents in combination, and in a perfectly neutral state, instantly start into an active existence, when exposed to the two electrical poles, it is necessary to assume principles, and take views of corpuscular action of a perfectly novel kind; and as the chief agents are invisible, and probably imponderable, no direct demonstrative evidence can be brought forward on the subject; and different hypotheses may in consequence be applied to it. In assuming the idea of two ethereal, subtile, elastic fluids, attractive of the particles of each other, and repulsive as to their own particles, capable of combining in different proportions with bodies, and according to their proportions giving them their specific qualities and rendering them equivalent

valent masses, it would be natural to refer the action of the poles to the repulsions of the substances combined with excess of one fluid, and the attractions of these united to the excess of the other fluid; and a history of the phænomena, not unsatisfactory to the reason, might in this way be made out; but as it is possible likewise to take an entirely different view of the subject, on the idea of the dependence of the results upon the primary attractive powers of the parts of the combination on a single subtile fluid, I shall not enter into any discussion upon this obscure part of theory, but I shall endeavour to clear the way for elucidations of it by stating some experimental results.

Some solution of nitrate of potassa was introduced into a glass basin of six inches in diameter, and large slips of paper, tinged with litmus and turmeric, were placed below the fluid, and connected with two pieces of foil of platinum; so that the indications of the formation of acid and alkali, in any part of the basin, by electricity, would be instant and distinct. The two pieces of foil were now connected with the poles of a voltaic battery: it was found that the alkali was developed only at the point or immediate surface of the negative platinum, and the acid in the same manner at the surface of the positive platinum; and they then gradually diffused themselves through the fluid in a circle round the conductors, and there was no appearance of any repulsions or attractions of the menstrua in the line of the circuit.

In various repetitions of this experiment the same result was obtained; the alkaline and acid matters were influenced in their direction only by currents produced by the disengaged oxygen or hydrogen, or the inclination of the vessel; in short, by mechanical causes only; and the same effects were produced on the test papers, as if a spherical piece of acid and an amalgam of potassium had been introduced in the places of the two poles.

Mr. Herschel has shown, by some elaborate and ingenious experiments in the last Bakerian Lecture, that an amalgam of potassium, containing so minute a portion as some hundred thousand parts of its weight is strongly attracted so as to occasion violent mechanical motion, by the negative pole in a voltaic arrangement: and if it be supposed that the fluid is divided into two zones, directly opposite in their powers to the poles of the battery, the virtual change may be regarded as taking place in the two extremities of these zones nearest the neutral point; so that by a series of decompositions and recompositions, the alkaline matters and hydrogen separate at one side, and oxygen, pure or in union, at the other.

In this way, the two electricities may be regarded as the transporters of the ponderable matters, which assume their own peculiar characters at the moment they arrive at the point of rest. I shall detail an experiment which I made under a different form some years ago, and which may assist the imagination in the conception of this singular and mysterious mode of action. A flat glass basin, 10 inches in diameter, was filled with water containing 1-2000ndth part of its weight of sulphate of potassa, in the bottom of which 30 or 40 separate globules of mercury, containing from 10 to 100 grains each, were placed without any regard to order; two wires of platinum from a battery of 1000 double plates, weakly charged, were made to connect the extremities of the water (passing to the bottom of the basin). As soon as the electrical communication was made, the globules of mercury in or near the current became instantly agitated; their negative poles became elongated, and approached either the positive pole of the battery, or the positive pole of the contiguous globules of mercury, and streams of oxide flowed with great rapidity from the positive toward the negative poles. No hydrogen appeared at the negative poles of the globules of mercury; but after the action had continued a few minutes, and was then suspended, there was an appearance of some minute globules, owing, as was proved by tests, to the formation and oxidation of potassium which had combined with the mercury, and which, as is evident from Mr. Herschel's researches, had given to that part of the globule in which it had combined its high electropositive qualities. When the connexion was again made, the same series of constant and violent motions took place; the elongated and negative extremities of every globule moving towards the positive surfaces, and undergoing continual oscillations; but on pouring a small quantity of muriatic acid into the water, so as to make it slightly acid, these phænomena ceased; the masses of mercury resumed their spherical form, hydrogen was given off from the negative surfaces, and all motion and agitation were at an end. The energy of the acid in this case being negative, may be considered as neutralizing the power of the potassium by its immediate contact, and as destroying all the phænomena of attraction by the positive pole.

In the numerous experiments that I made in 1806, on the transfer of acids to the positive pole and of alkalies to the negative pole, there were similar instances in which masses of acid or alkaline matters, by exerting their own peculiar energies, prevented the accumulation of the antagonist elements at their points of rest, so as to destroy, or materially weaken, their power of motion or transport. For instance, in attempting

ing to transfer baryta from the positive to the negative pole, the negative pole being plunged in sulphuric acid, or sulphuric acid to the positive pole, the negative being plunged in a solution of baryta, the re-agents were neutralized, and formed insoluble precipitates at the point of union of the menstrua; and no baryta reached the negative, and no sulphuric acid the positive pole.

With muriatic acid and salts of silver the case was the same. And when acids and alkalies, forming soluble compounds, were used in similar experiments, a great length of time was required, proportional in some measure to their masses, before a particle of acid reached the positive, or of alkali the negative pole; and the result was not destroyed till after the intermediate combination had taken place to a considerable extent; proving the phænomena of continued decompositions and recompositions, and showing that the electrical and chemical phænomena are of the same order, and produced by the same cause.

In the Bakerian Lecture for 1806, I proposed the electrical powers, or the forces required to disunite the elements of bodies, as a test or measure of the intensity of chemical union. By the use of the multiplier it would be now easy to apply this test; and *accurate* researches on the connexion of what may be called the electro-dynamic relations of bodies to their combining masses or proportional numbers, will be the first step towards fixing chemistry on the permanent foundation of the mathematical sciences.

I could enter into some other general views of the pure scientific relations of this subject, and its connexion with thermo-electricity and the phænomena of cohesion; but having already taken up so much of the time of the Society, I shall defer what I have to say on these subjects to another occasion, and I shall conclude with a few practical observations.

A great variety of experiments made in different parts of the world has proved the full efficacy of the electro-chemical means of preserving metals, particularly the copper sheathing of ships; but a hope I had once indulged, that the peculiar electrical state would prevent the adhesion of weeds or insects has not been realized; protected ships have often indeed returned after long voyages perfectly bright*, and cleaner than unprotected ships, yet this is not always the case; and though the *whole* of the copper may be preserved from chemical solution in steam vessels by these means, yet they must be adopted in common ships only, so as to preserve a portion,—so ap-

* The Carnebrea Castle.

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plied as to suffer a certain solution of the copper*; and an absolute remedy for adhesions, is to be sought for by other more refined means of protection, and which appear to be indicated by these researches.

The nails used in ships are an alloy of copper and tin, which I find is slightly negative with respect to copper, and it is on these nails that the first adhesions uniformly take place: a slightly positive and slightly decomposable alloy would probably prevent this effect, and I have made some experiments favourable to the idea.

In general, all changes in metals which would indicate the power of chemical attraction, are easily determined by electrical means. Thus I found copper hardened by hammering negative to rolled copper; copper (to use the technical language of manufacturers) both *overpoled* and *underpoled*, containing in one case probably a little charcoal and in the other a little oxide, negative to pure copper. A specimen of brittle copper, put into my hands by Mr. Vivian, but in which no impurity could be detected, was negative with respect to soft copper.

In general, very minute quantities of the oxidable metals render the alloy positive, unless it becomes harder, in which case it is generally negative. As I have mentioned before, amalgams of the oxidable metals are usually positive, not only to mercury, but even to the pure metals.

There are probably few chemical operations which electrical changes do not influence, and either increase or modify. In the rusting of iron, for instance, the oxide formed by the contact of moisture becomes the negative surface, and exalts the oxidability of the mass of metallic iron, and the rust consequently extends in a circle.

The precipitations of metals have been already traced to causes of this kind, and many metallic solutions must belong to the same order of phænomena.

I have pointed out in former papers some of the cases of electro-chemical protection, which I have no doubt, when the principles are well understood, will be generally adopted; and others are constantly occurring. I shall mention one,—the preservation of the iron boilers of steam engines by introdu-

* A common cause of adhesions of weeds or shell fish, is the oxide of iron formed and deposited round the protectors. In the only experiment in which zinc has been employed for this purpose in actual service, the ship returned after two voyages to the West Indies, and one to Quebec, perfectly clean.

The experiment was made by Mr. Lawrence, of Lombard-street, who in his letter to me states that the rudder, which was not protected, had corroded in the usual manner.

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cing a piece of zinc or tin. This in the case of steam boats, particularly when salt water is used, may be of the greatest advantage, and prevent the danger of explosion, which generally arises from the wear of one part of the boiler.

Another application of importance which may be made, is the prevention of the wear of the paddles or wheels, which are rapidly dissolved by salt water.

But I will conclude. Whenever a principle or discovery involves or unfolds a law of nature, its applications are almost inexhaustible; and however abstracted it may appear, it is sooner or later employed for common purposes of the arts and the common uses of life.

XLI. *Outlines of a Philosophical Inquiry into the Nature and Properties of the Blood; being the Substance of three Lectures on that Subject delivered at the Gresham Institution during Michaelmas Term* 1826. *By* John Spurgin, *M.D. Fellow of the Royal College of Physicians of London, and of the Cambridge Philosophical Society* *.

THE plan pursued in these Lectures, in regard to the mode and style of their composition, being intended rather for the general class of intelligent hearers, than for the medical profession exclusively, it may readily be conceived that a departure from what might be termed the usual method of treating a physiological subject like that of the blood, was almost unavoidable, at the same time that it might be deemed in some degree justifiable. The philosophical and abstract reasoning upon the nature and properties of this fluid, directed and limited as it is, by the facts, the observations, and the experiments which are adduced concerning it, will not, it is hoped, be thought unworthy of attention, or destitute of interest and utility.

To confine the Lectures to a bare enumeration of facts, to the exclusion of all reasoning, was not so much the object in their composition, as to draw conclusions from them, that might lead to further inquiry on the same subject; at the same time that they interested the hearers; and as the Lectures were not drawn up with any view to publication in their present shape, they may perhaps be entitled to indulgence for the novelty of the method adopted, in the investigation of this important part of the Animal Economy.

To enter upon a course of investigation into the economy of the animal kingdom, or in other words, to bring the human

* Communicated by the Author.

human faculties into exercise with the view to discover the uses and ends of all the parts which compose this kingdom; more qualifications are required than might at first sight be imagined: for not only is it necessary to possess a thorough knowledge of the anatomy of the body, but likewise to be conversant with several other highly important branches of science, both mathematical and physical, and to be initiated moreover into that sort of abstract or philosophical reasoning, which enables us to discern the difference between a cause and its effect; or to perceive the relationship that subsists between a substance, and the forces and powers which it may be the medium of manifesting.

The investigation then of the animal economy requiring so many aids, we must not be surprised at the slow progress of our knowledge concerning it, nor ridicule the various strange hypotheses and fancies of our ancestors; still less ought we to contend with any one of our own day for or against an opinion, as if it were an empire; because if we are guided by experience, and the clear deductions of reason, the truth will in all probability be eventually attained, to the dispersion of error. But as I am precluded from entering upon such an investigation, or upon a consideration of the animal economy, to any great extent, and can only take up the subject in a very general and cursory manner, I have ventured to draw your attention to the most important part of this economy, viz., the blood: because the animal system regards the blood as its common fountain and source; and in a philosophical point of view, it may be said to be a general principle pervading and entering into every part and portion of the body.

I should be extremely unwilling to offer any thing to your consideration, which might not prove either interesting or useful to you in some degree; but if the rule which insists on an enumeration of facts, to the exclusion of all reasoning, and which is too generally acted upon, be taken as the measure of this interest or utility; the prefatory remarks with which I have set out, and which have been drawn up in opposition to the rule just alluded to, will, I fear, prove insipid to some, and useless to others: but as in the pursuit of my plan there is an abundance of facts to bring forward, and as some indulgence is claimed in an Introductory Lecture, I trust you will find the general rule complied with in the sequel, whilst I avail myself of your indulgence at the outset.

If therefore we in the first place take an abstract or more philosophical view of this most extraordinary fluid, the blood; we may discern in it, as in a type, all the individual parts of the animal economy; seeing that neither the solids nor the remaining fluids of the body are derived to it, from any other source

source than from the blood; we may also be enabled to see in what manner, and under what sense, it may be regarded as the life of the body; inasmuch as our experience proves to us, that the state and condition of animal life depends upon the *nature*, *constitution*, *determination*, *continuity* and *quantity* of the blood; and that under the same view, its vessels, or the arteries and veins, are neither more nor less than its determinations, composing in fact for the most part the entire body. Moreover, when the proofs to be derived from the best chemical authorities are adduced, of the variety of elements, whether ultimate or proximate, that enter into the composition of the blood, it will be seen that this fluid is in fact a complex of many things existing in the world, and as it were, a seminary and storehouse of whatsoever exists in the body; for it contains, as will be shown in the next lecture, salts of various kinds, both fixed and volatile, and the gaseous elements,—as oxygen, hydrogen, and azote; in short, numerous products from the three kingdoms of nature,—the animal, vegetable, and mineral; and imbibes also those things which the atmosphere conveys in its bosom or holds in solution; for by means of the lungs or respiratory apparatus, it exposes itself to the air, to be enriched with its treasures.

Now as the blood contains in itself, in this compendious manner, so many of the productions of the whole world, and of its several kingdoms; may it not be allowable to infer that these were all created for this end,—namely, to administer to its composition and continual renewal? For it may be rationally argued, that if all things were created for the sake of man, and to afford him the means of subsistence and thence of life; then all things were created for the sake of the blood, which is the parent and nourisher of every part of the body: *for nothing exists in the body which did not first exist in the blood.*

So true is this, that if the texture of any muscle or gland, of which the viscera are for the most part compounded, be divided into its minutest parts, it will be found to consist chiefly of vessels containing blood, and of fibres or nerves containing, or conducting, without doubt, a corresponding and more eminent fluid or blood. And even those parts which do not appear to consist of such vessels,—as the bony, cartilaginous and tendinous structures,—will nevertheless be found in their soft and infant state, or during infancy, to be similarly composed, as experience can prove. The blood then is not only a treasury and storehouse enriched with all the various productions of nature, and thence enabled to bestow on the body, as its offspring, whatever it requires for necessity and use;

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but is also, as it were, its all in all; and in it are contained the means which enable man to live in a corporeal form in this outward world, in the manner we behold.

But in order to our completing the circle of investigation upon the blood, and thence obtaining a true knowledge and correct doctrine respecting it: a knowledge of those things which enter into its composition and constitution is indispensable, as also an examination of all the viscera, members, organs and tunics, which are vivified by its passage through them; for whilst the nature of these is unknown, and their modes of existence and action, the nature of the blood remains unknown also. It is impossible for us to enjoy any clear ideas upon any subject, if certain parts of that subject remain unknown or obscure to us: a full and complete idea of any subject, can only be attained from a knowledge of every particular which the subject involves; and consequently, our knowledge of the nature of the blood can only keep pace with that of the things which enter into its composition, and of those in which it is contained, as the blood-vessels and organs composing the body.

From these remarks it may sufficiently appear how many sciences are included in that of the blood:—anatomy, medicine, chemistry, and natural philosophy, with their respective sub-divisions are evidently so; and not only these, but even psychology is requisite, for the mind or mental powers suffer according to the state of the blood; and the blood, again, is under the influence of the passions of the mind:—in a word, every science that treats of the substances of the world and the powers of nature ought to be consulted. Such considerations as these enable us, moreover, to discern the ground and reason of man's not being called into existence till all the kingdoms of nature were finished. The world and nature seem to have concentrated themselves in him; that in him, as in a microcosm, the whole universe from first to last might be contemplated.

It is expedient on all occasions to keep close to experience, and also to follow the order of nature; according to which, a distinct idea is always preceded by an obscure one, and a particular idea by a general one: for we never perceive any thing distinctly, unless we deduce it from, or refer it to, some common source, and universal principle. For such is the condition of our mind and senses in their advancement to perfection and subsequent actions. We are born densely ignorant and insensible; it is only by degrees that the organs are opened, as it were: the images and notions which we first conceive are extremely obscure, insomuch that, so to speak, the whole

universe

universe is presented to the eyes as a single indistinct thing, a shapeless chaos: yet all things in process of time become more distinct, and at length make their way to the rational faculty of the mind;—thus are we a long time in becoming rational.

Whether we have discovered the truth or not, respecting any subject, is easily ascertained; for all experience will then spontaneously bear testimony in its favour, so likewise will every rule of true philosophy: for when truth is at hand, nothing whatever refuses it its suffrage; hence it immediately manifests itself, and commands belief, or, as is commonly expressed, presents itself naked.

Nothing can introduce us to the causes of things, or to truth, but experience alone: for when the mind or contemplative faculty is left to expatiate without restraint, or without experience for its guide, how easily does it fall into error, and go stumbling on from one absurdity to another; and if it then looks to experience for confirmation and patronage, the attempt will be wholly useless and vain. To consult experience after assuming our principles is an erroneous mode of proceeding: we should on the contrary consult experience first, and deduce our principles from it; when we are led away by reasoning alone, we are not unlike those who, with their eyes blindfolded, as is sometimes practised in childish sport, believe themselves to be walking in a straight direction, but who on the removal of the bandage find that they have wandered greatly from the path, and that if they had continued their blind progress they would have arrived by a circular course, at a place the very opposite to that of their destination.

But it may be inquired whether we have at the present day a sufficient store of experience or of facts to enable us to discover Nature's secrets so successfully as the above considerations would lead us to expect, without its being necessary to suffer our minds to wander into the wild field of conjecture unrestrained by experience. It cannot be denied that our experience or our knowledge of any one individual thing, let it even be enriched and increased by the accumulated experience of ages, can never suffice to complete the investigation of the subject to which it relates, to its very and inmost causes. But if all that is known, or all our general experience in anatomy, medicine, chemistry, physics, and the other natural sciences, be called to our aid in the exploration and investigation of any individual thing,—as in this instance in the investigation of the blood, we may affirm that we are at this day sufficiently provided for the purpose.

When we confine our experimental research to a single object,

ject, as to the blood, or to a muscle, or a gland; this research can never be so complete as to exhaust and to display all the hidden qualities of that object. Let us take the blood for an example: The experiments which have been made upon it only inform us that its colour is of different degrees of redness; that it is heavier than water; that it sinks to the bottom of the serum; that it is of a gentle and almost uniform warmth in the body; and that it contains salts both fixed and volatile, of several kinds, besides other things, such as albumen, and fibrin, which are termed animal matter; and variable proportions of water. But these experiments alone do not inform us whence its redness, its gravity, and its heat, derive their origin; nor in what way the products to be obtained from it by distillation, or by means of chemical analysis, are preserved therein in that peculiar combination and form that renders the blood such an homogeneous and simple fluid as it appears to be in its natural and fluid state. These latter points must be regarded as so many accidents and essentials, the knowledge of which is only to be sought for and obtained in common or more general experience, or in our experience as taken in its whole compass and course. For we do not hesitate to assert, nor are we afraid to maintain, that whenever a subject is defined and determined by occult qualities, it remains as obscure and unintelligible as if no definition or description had ever been given; in like manner as we stop at the very threshold of the science of angiology, or of the circulation of the blood, if we do not learn the whole anatomy of the body and of all its viscera; that is, unless we closely pursue the blood into all the recesses into which it flows.

The case is similar in all other instances, whether in anatomy, or physics. Thus, if we would investigate the causes of the action of a muscle or moving fibre, our labour will be in vain, unless, in addition to our more confined experience or knowledge of the muscle or fibre itself, as to its particular form or situation, we are at the same time acquainted with many of the particulars relating to the rest in the body, and likewise with those relating to the blood, its arteries, and heart, to the nerves, ganglia of nerves, medulla oblongata and medulla spinalis, to the cerebellum, the cerebrum, and to many of the members, organs, and tunics endowed with the faculty of muscular motion: and not only so, but we ought also to know the chief particulars relating to those parts of physics and mechanics, which treat of forces, elasticity, motion, and several other subjects.

Thus it may be seen, how, from a knowledge or experience of the particulars involved in any one subject, our notions and ideas

ideas of that subject are but very obscure and indistinct; but how that in process of time and by diligent study, these ideas may be rendered more distinct and clear, by means of the general experience we may have at length acquired: for as we observed above, it takes a long time for man to become rational; or in other words, for the rational faculty of man to become stored with those truths which are indispensable to his becoming a truly rational or intelligent being.

We cannot help bringing to your notice the connexion, communion, and mutual respect existing between all things of the world and nature;—for does not one science meet and enlarge our apprehension of another, and every new acquisition afford an explanation to what preceded? By many and various facts judiciously associated and mutually compared, our ideas are illustrated and our reason illuminated; for it is only by degrees that the mind disperses the shadows and clouds of ignorance and prejudice, and emerges thence into light. Still, however, there is a danger of our relying upon our thorough knowledge or experience of some single subject, as a means of our extending our reasoning to other things with which it may have only a remote connexion. Examples of this are too abundant in our own day: for how many are there who are well skilled in one particular science, and who would investigate or measure every other, by it alone. Thus the chemist may look for nothing but chemical affinities and decompositions in the three kingdoms of nature; the mathematician for nothing but gravitating tendency, polarity, centripetal and centrifugal forces; the anatomist, for nothing but structure and form; the painter, for nothing but colour, light and shade; the musician, for nothing but harmony and sound; and the physician, for nothing but irritability, and numerous other technicalities. Drawing general conclusions from such confined sources, how dexterously does such limited experience favour the mind in all its reveries, and how obstinately does it withstand the objections advanced by the truly rational antagonist? The reason of this is, because no fact can exist which may not be placed in some part or other of different series of ratiocinations; just as one syllable, word, or phrase, may enter into and form a part of innumerable sentences and discourses; one idea of innumerable series of thoughts, and one colour of innumerable pictures. One thing may always be inserted on another, as branches are by the gardener; and thus *a false inference may be grafted on a certain fact*, as a wild fruit-tree on the legitimate growth of the orchard.

To avoid, therefore, being made the dupes of appearances, we should never yield our assent to any theory, without its having the concurrence of common or general experience; or

unless

unless all the facts which can be brought to bear upon it unite their suffrages in its favour; that is, unless the final conclusions are connected with and confirmed by the mediate links, throughout their whole progression. To me it appears that there is no other possible way for an edifice to be constructed, or for a system of philosophy to be formed, which posterity shall acknowledge, on the superadded testimony of thousands of new experimental discoveries, to rest on a solid foundation, and it shall no longer be necessary for every age to be perpetually erecting new structures on the ruins of the former.

If a time shall ever arrive when the human mind will be enabled to deduce an entire series of conclusions from the facts and general experience with which it can be furnished, so as to build up a more harmonious and consistent philosophy than we at present enjoy; the facts themselves and our general experience must be of that definite and indisputable kind that will impress on our minds a conviction of their immutability. To such a state of things are we undoubtedly advancing; but it is impossible to say how remote we are from this state at present. Every science requires of its cultivators a rejection of hypothesis and an attainment of certainty, to such a degree almost as to admit of calculation: in no instance is this more apparent than in chemistry. Consequently we may with justice aver, that we are advancing to a period when the human mind will be enabled to philosophize more consistently and harmoniously than heretofore; more especially as we have good reasons for supposing that the human mind, regarded in itself and as to the complex of its astonishing faculties, is as perfect in one age as in another; is as capable of instruction at the infancy of a state as at its maturity: the only requisites being, good materials for its development, and well-established facts as things upon which it can be exercised;—it being in this respect exactly similar to the human body, which is as perfect at this day as it was ages ago; as capable of imbibing nourishment in the peasant as in the prince,—the only requisites being wholesome food for its growth and repair, and active pursuits to preserve it in vigour. An analogy of this kind may also be seen to exist, between the gradual advance of mankind from barbarism to civilization, and the gradual progress of the mind from ignorance to intelligence: for whilst history in recording the one, interests us with the extraordinary feats of mighty heroes and conquerors; so do the volumes of literature in containing the other, astonish us with the vast manifestations of mental power exhibited by profound reasoners and skilful experimenters.

At no period of the world was the human mind so qualified

fied for bringing its various faculties into full exercise, and to a more complete exhibition of its inherent powers, than at the present:—for never was it so free to act its part in thinking, judging, and deciding upon, any matter: never did it enjoy such a vast accumulated store of experience, for its basis and direction; never did it exhibit such a thirst after and such a relish for knowledge; never did it betray such a disposition to scrutinize the theories, doctrines and traditions which have so long held her in bondage, as at the present day. And surely if it be disposed for what is good, as well as for what is true, its gratitude to the country and age which is now yielding to its empire, will increase with the auspicious extension of its dominion. In every department of science it must be admitted that such sentiments as the above are now tacitly acknowledged; and if they prevail to any benefit, we think we shall not err in saying, that this benefit will in no instance be more apparent than in that of medicine. This science is classed, and rightly too, among the liberal sciences; and we hope it will not only continue to hold so respectable a rank among them, but whilst conducing to the common good of mankind, exhibit among its cultivators a good-will and fellow-feeling which will prompt them to regard each other's sentiments, upon every subject, with mutual candour and forbearance;—for where the good of society is the object, there the heart, the head, and the hands, will conspire to promote it.

It being then one of the purposes of this Institution to deliver lectures on some subject of medical science to any individuals who may wish to acquire some knowledge respecting it, it is quite impossible to convey such knowledge in any other than a very general way indeed; wherefore I have thought it expedient, from the circumstance of my merely occupying Dr. Stanger's place during his absence from town, to enter upon a subject, which, though the most general of all, and thence perhaps the least understood, yet involves so many particulars of interest as well as of general experience, that the subject may be regarded as worthy of your attention and consideration.

The subject I allude to, is the blood. To a thorough knowledge of which so many requisites are indispensable, that we intend only to bring forward common facts, or general experience, in framing our doctrine concerning it.

But as the time will not allow of my bringing forward the results of the experiments to which this fluid has been subjected, I must beg to defer this *material* part of my subject to the next lecture. And at our third meeting I hope to have it in my power to proceed to consider two of the most interesting and remarkable properties of the blood,—viz. its FLUIDITY and VITALITY.

[To be continued.]

XLII. *On*

XLII. *On Contemporaneous Meteorological Observations, as proposed by the Royal Society of Edinburgh.* By THOMAS SQUIRE, *Esq.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen.

I HEREWITH send you a Meteorological Table containing the monthly means of the barometer, thermometer, &c. as obtained from daily observations made at 8 A.M. at Epping, during the year 1826, and also the depth of rain for each month of the last five years. I have also added another table of the hourly observations made at this place on the 17th July, 1826, agreeably to the wish of the Royal Society of Edinburgh. In this latter table I have moreover given the computed altitudes of my barometer at Epping above that of Mr. Bevan's at Leighton-Buzzard; the *mean* of which agrees so well with *that* obtained from similar observations made at the two places in 1821, as given in the 58th vol. of the Philosophical Magazine, from the computations of Mr. Bevan, jun., that for this reason I was induced to trouble you with them on the present occasion.

It may not be improper to say something respecting the instruments used in these observations, as relates to their construction and locality,—particulars which ought not to be lost sight of, as a knowledge of such *minutiæ* are sometimes of great importance, especially when any deductions are intended to be made from such observations.

First, The barometer is a portable one, of superior workmanship; it has a capacious cistern and a large tube, but there is no adjustment for the change of level in the mercury of the cistern, neither is the neutral point nor the ratio of the tube to the cistern marked upon it. These are certainly imperfections; but as the diameter of the cistern is very great compared with that of the column of mercury, and as the point of *zero* is, most probably, between 29 and 30, no great errors could have arisen from these causes, in the hourly observations, under the then atmospheric pressure. This barometer with its attached thermometer hang in an open situation, with the surface of the mercury in the basin 12 feet from the ground, free from the rays of the sun, and from the effects of artificial heat.

The external thermometer is freely exposed to the air in the shade, with a N.W. aspect; the bulb is perfectly bare, and it is so situated as not to be affected by direct radiation:—its height from the ground is about 5 feet.

The

The rain gauge is of the same kind as the one described by Luke Howard, Esq. in his elaborate work on the Climate of London; it stands in an open situation, about 7 feet from the ground, and at such a distance from any tree or building as to prevent, in the least degree, the quantity of precipitation being affected by such a cause. The rain is measured daily, as often as any falls; and in the summer season, during showery weather with bright intervals, this is done more frequently, for the purpose of guarding against any diminution arising from the effects of evaporation.

In the 2nd Table, containing the hourly observations of the barometer, &c., the same instruments were used on this occasion as in the daily observations, with the addition of De Luc's whalebone hygrometer and an horizontal self-registering spirit thermometer; they were both exposed to the open air in the shade, the former 5 feet from the ground and the latter 2 feet.

A Meteorological Table for the Year 1826, *together with the Depth of Rain for the last Six Years.*

Epping: Lat. 51° 41′ 41″·6 N. Long. 27″ E. Time of observation 8 A.M.

Months.	Mean of Barometer	Rel. State of Bar.	Attached Thermometer.	External Thermometer.	Wind.				Rel. State of Wind.	Depth of Rain.				
					N.	E.	S.	W.		1822.	1823.	1824.	1825.	1826.
1826.														
Jan....	29·672	6	34·1	29·1	25	50	28	21	44	·418	1·777	·910	1·010	·170
Feb. .	29·599	–2	43·4	39·9	2	16	57	33	64	1·358	3·318	2·468	·935	2·066
March	29·634	–1	43·9	39·7	34	32	34	24	67	1·517	1·292	2·844	1·321	1·950
April .	29·638	1	49·6	47·5	33	10	22	25	62	2·688	1·835	1·930	2·248	1·157
May .	29·681	1	52·7	51·4	80	28	8	8	59	1·210	1·007	3·775	2·534	2·432
June .	29·904	7	63·5	62·7	50	35	14	21	56	·961	1·633	5·765	1·405	·410
July .	29·657	1	66·6	65·1	22	22	35	45	58	3·011	1·938	1·782	·008	2·650
Aug. .	29·658	5	66·5	64·0	18	22	41	43	64	1·388	2·577	2·620	2·775	1·638
Sept. .	29·565	2	60·0	56·5	23	38	37	22	63	·764	2·201	4·092	3·381	3·471
Oct. .	29·582	2	55·0	50·9	15	30	44	35	50	3·824	2·558	2·878	3·711	2·878
Nov. .	29·463	3	42·7	38·0	47	8	23	42	59	3·847	2·122	3·902	3·863	2·998
Dec. .	29·531	7	44·0	40·2	29	24	40	31	42	1·646	3·081	3·272	3·471	1·679
Mean	29·632	32	51·8	48·8	32	26	32	29	57	22·632	25·339	36·238	26·662	23·499

For the contemporaneous Meteorological Observations.—July 17, 1826.

Meteorological Observations made at Epping. Latitude 51° 41′ 41″·6. Longitude 27 seconds in time east of Greenwich. Altitude above the level of the sea 396 feet, subject to future correction.

Hours.	Barometer. Height.	Barometer. Temp.	Thermometer. Mercury 5 feet from the ground.	Thermometer. Spirit 2 feet from the ground.	De Luc's Hygrom.	Wind. N.	Wind. E.	Wind. S.	Wind. W.	State of Wind.	The Altitude of Epping above Leighton, in Feet.	Remarks on the Apparent State of the Atmosphere.
A.M. 1	29·594	64	52	50	65	2			2	L		A cloudless sky.
2	29·582	63	50	49	70	1			3	VL		Bright. Some small in the region of twilight.
3	29·588	63	50	49	74	1			3	VL		A cloudless sky.
4	29·599	62	49	48	77	1			3	L	108·2940	Ditto.
5	29·611	62	51	49	76	1			3	L	112·6950	Ditto.
6	29·621	62	53	51	74	1			3	RB	111·7540	Ditto.
7	29·630	61	56	54	73	1			3	RB	108·6198	Ditto.
8	29·645	61	59	57	67	1			3	RB	101·3137	Sky, with nascent rising to windward.
9	29·645	61	63	60	62	1			3	RB	101·3664	Sky, with broken floating in the wind.
10	29·638	61	64	62	60	1			3	RB	110·2844	Ditto.
11	29·651	62	66	63	58	1			3	RB	104·4094	Ditto.
12	29·660	62	68	65	55	1			3	RB	97·4031	Sky with light .
13	29·652	64	71	68	53	1			3	RB	103·0700	Bright, a few and small .
14	29·659	65	72	70	51	1			3	RB	94·5114	The character and appearance of the sky nearly the
15	29·669	66	73	71	50	1			3	RB	91·5498	Bright, a few small . [the same as last observ.
16	29·669	67	73	72	49	1			3	L	90·2377	Ditto and ditto.
17	29·669	68	73	72	49	1			3	L	89·8332	Bright, some and a few small .
18	29·662	69	73	72	48	1			3	VL	97·7425	Bright. *Parhelia*, that on the right of the sun very
19	29·661	70	72	71	49	1			3	VL	102·1069	Bright, a few . [bright.
20	26·661	69	69	67	52	1			3	NO	100·9811	Bright, some , ☉ set of a yellowish hue.
21	29·661	68	67	64	53			3	1	NO	122·0562	Bright, some to the W.—N.
22	29·670	68	65	63	54			4		VL	98·2027	Much and .
23	29·664	67	62	61	63			4		VL	———	Much , stretching from N. to S. [the ☽.
24	29·658	67	60	59	67			4		NO	102·4437	Less clouds than in last observation; towards

The observations here recorded have been made with great care, and as near the stated times as possible.

However desirable it may be to institute a general plan for the purpose of barometrical measurement, yet it is clear to every one, that instruments of the very best construction only can be used on such an occasion, with any prospect of success, in this interesting and useful branch of inquiry. And I have moreover to regret, that it is not at present in my power to give to the above barometrical observations that degree of accuracy I could wish, for the want of the necessary corrections for the relative capacity of the cistern and tube, and capillary action, &c. It was my intention to have sent you the annual means of the atmospheric pressure for the last five years, as obtained from daily observations made at this place during that period; but these I shall defer till such time as I can with certainty apply to them the requisite corrections and reductions, by a comparison with a barometer of the most approved construction.

It appears from the highly valuable Essays on Meteorology by J. F. Daniell, Esq. that this gentleman has paid more than usual attention to the manufacture of barometers; and the one that has been made for the Royal Society under his superintendence, is doubtless superior to any thing of the kind known in this country.

For the more certain verification of the accuracy and goodness of barometers intended for philosophical experiments, would it not be proper that all such instruments were sent, for a time, to the Royal Society's apartments, for the purpose of comparison with this standard instrument? as from the rapid advances and important discoveries that are now making in the long neglected science of Meteorology, this seems almost as necessary as it is to send chronometers to the Observatory at Greenwich, with the intention of determining the regularity of their rates, and thereby their fitness for ascertaining the longitude with certainty.

I will here just remark that on looking over Mr. Weekes's Table Phil. Mag. vol. lxviii. p. 316. I am of opinion that this gentleman's barometrical numbers for Sandwich must be very incorrect; for on comparing these observations with those made at the same time at Leighton Buzzard by Mr. Bevan, it appears that Mr. W.'s barometer was in general lower than Mr. B.'s, but at 2 P.M. Mr. W.'s barometer was at 29·84, and at 3 it seems to have suddenly fallen to 29 inches, and there continued to the end of his observations. Now from a comparison of other observations made on the same day in different parts of the country, it does not appear that

 any

any change like this, took place in the atmospheric pressure, on the 17th of *July* last, over any part of the British isles. And therefore, setting aside the relative situations of the two places it must be inferred that either Mr. Weekes's barometer is extremely defective, or that the observations were made without sufficient regard to accuracy, and for that reason are unfit for the purpose intended.

I have with great pleasure read Mr. Daniell's excellent work, entitled "Meteorological Essays," and in justice I can truly say that great credit is due to that gentleman, for his laborious and scientific researches relative to meteorology:—his valuable illustrations of the general laws and phænomena of our atmosphere;—his unremitted attention to the improving and perfecting of the various instruments connected with this branch of philosophy, entitle him to the highest praise and thanks of all true lovers of science.

I remain, Gentlemen, yours truly,

Epping, Jan. 4th 1827. THOMAS SQUIRE.

XLIII. *On the expected Occultation of Venus in February. By* THOMAS SQUIRE, *Esq.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

ON reading Mr. Baily's introductory remarks to his valuable list of moon-culminating stars, he says, "You will observe that I have inserted Jupiter and Saturn, when they are near the moon, and when their motion is *retrograde:* and also Venus on the day of her occultation in February."

Now with respect to the ☌ of ♀ and the ☽, on the morning of the 22nd of February, it appears from the result of computations which I made relative to this ☌, for Moore's Almanack of the present year, that this will not prove to be an occultation at Greenwich. For at the time of visible ☌, (which happens at 33^m after 9, apparent time,) the apparent latitude of ♀ exceeds that of the moon $17' 57''$; and if from this we take the sum of the apparent semidiameters of the moon and Venus, it will leave $1' 29''\cdot4$, for the distance of the ☽'s northern limb from the southern limb of Venus. But the nearest approximation in this respect will be $1' 8''\cdot4$, as seen from the Royal Observatory.

Hence we may conclude that there will not only be no occultation at Greenwich, but that this will also be the case in every other part of England.

It is to be hoped the atmosphere will prove favourable for verifying

verifying the accuracy of the above calculations. Venus is at this time approaching very near the point of her greatest elongation, and will pass the meridian about 32 minutes before the time of visible conjunction, at an altitude of about 16° 51′ above the horizon of Greenwich. Yours truly,

Epping, Jan. 6th, 1827. THOMAS SQUIRE.

XLIV. *Some Account of an Orang Outang of remarkable Height found on the Island of Sumatra; together with a Description of certain Remains of this Animal, presented to the Asiatic Society by* Capt. Cornfoot, *and at present contained in its Museum. By* CLARKE ABEL, *M.D. F.R.S. &c. &c., and Member of the Asiatic Society of Calcutta*.*

IN the paper which I had the honour of reading to the Asiatic Society on the evening of the 5th of January last, I endeavoured to be as comprehensive as possible, in relation to the published histories of large man-like apes, and to the many speculations of philosophers respecting them; and in order the better to accomplish my purpose, I divided my subject under the following heads: First, I gave an account of what particulars I had been able to collect of the circumstances which attended the capture of the Sumatran animal: Secondly, I gave the amplest description in my power, of its different remains, as they were before the Society: Thirdly, I adduced a description of Wurmb's animal, as described in the Batavian Transactions, for the purpose of showing its identity with the Sumatran Orang Outang: Fourthly, I brought forward a description of the small Orang Outang of Borneo, for the purpose of showing its relationship to the two former animals, and for the better examining the opinion of the Baron Cuvier, that it is only the young one of Wurmb's, and consequently of the Sumatran animal: and lastly, I quoted some notices of very large man-like apes contained in the works of the older travellers, and attempted to determine to which of these the Sumatran Orang should be referred. The essay which I read to the Society was prepared in haste, and from imperfect materials; and although it might, perhaps, be suited to its principal object, that of exciting inquiry, it was certainly unfit for publication. For this reason, and because those who are likely to be chiefly interested in this communication will be better satisfied with facts than opinions, I shall at present limit myself to an account of those particulars of the appearance of the animal when alive which are best authenti-

* From the Asiatic Researches, vol. xv. p 489.

cated,

cated, and of the circumstances that attended his capture, as they have been collected from the persons who took him, and conclude with a description of such parts of his body as are preserved in the museum of the Asiatic Society.

Capture of the animal.

The following short history of the circumstances under which the animal was found, and of the mode of taking him, is drawn up from accounts which were furnished to me either directly or indirectly by persons concerned in his capture. A boat party under the command of Messrs. Craygyman and Fish, officers of the brig Mary Anne Sophia having landed to procure water at a place called Ramboom near Touraman, on the N.W. coast of Sumatra, on a spot where there was much cultivated ground and but few trees, discovered on one of these a gigantic animal of the monkey tribe. On the approach of the party he came to the ground, and when pursued sought refuge in another tree at some distance, exhibiting as he moved, the appearance of a tall man-like figure covered with shining brown hair, walking erect with a waddling gait, but sometimes accelerating his motion with his hands, and occasionally impelling himself forward with the bough of a tree. His motion on the ground was plainly not his natural mode of progression, for even when assisted by his hands or a stick, it was slow and vacillating: it was necessary to see him amongst trees in order to estimate his agility and strength. On being driven to a small clump, he gained by one spring a very lofty branch, and bounded from one branch to another with the ease and alacrity of a common monkey. Had the country been covered with wood, it would have been almost impossible to prevent his escape, as his mode of travelling from one tree to another is described to be as rapid as the progress of a swift horse. Even amidst the few trees that were on the spot, his movements were so quick that it was very difficult to obtain a settled aim; and it was only by cutting down one tree after another, that his pursuers by confining him within a very limited range, were enabled to destroy him by several successive shots, some of which penetrated his body and wounded his viscera. Having received five balls, his exertions relaxed, and reclining exhausted on one of the branches of a tree, he vomited a considerable quantity of blood. The ammunition of the hunters being by this time expended, they were obliged to fell the tree in order to obtain him, and did this in full confidence that his power was so far gone that they could secure him without trouble, but were astonished as the tree was falling to see him effect his retreat to

to another, with apparently undiminished vigour. In fact, they were obliged to cut down all the trees before they could drive him to combat his enemies on the ground, against whom he still exhibited surprising strength and agility, although he was at length overpowered by numbers, and destroyed by the thrusts of spears and the blows of stones and other missiles. When nearly in a dying state, he seized a spear made of a supple wood which would have withstood the strength of the stoutest man, and shivered it in pieces; in the words of the narrator, he broke it as if it had been a carrot. It is stated by those who aided in his death, that the human-like expression of his countenance, and piteous manner of placing his hands over his wounds, distressed their feelings, and almost made them question the nature of the act they were committing. When dead, both natives and Europeans contemplated his figure with amazement. His stature at the lowest computation was upwards of six feet; at the highest it was nearly eight; but it will afterwards be seen that it was probably about seven. In the following description, which I give in the words of my informant, many of my readers will detect some of those external conformations which distinguish the young eastern Orang Outangs that have been seen in Europe. The only part of the description in which the imagination seems to have injured the fidelity of the portrait, regards the prominence of the nose and size of the eyes, neither of which are verified by the integuments of the animal's head. "The animal was nearly eight feet high, and had a well-proportioned body, with a fine broad expanded chest and narrow waist. His head also was in due proportion to his body; the eyes were large, the nose prominent, and the mouth much more capacious than the mouth of a man. His chin was fringed from the extremity of one ear to the other, with a beard that curled neatly on each side, and formed altogether an ornamental rather than a frightful appendage to his visage. His arms were very long even in proportion to his height, and in relation to the arms of men; but his legs were in some respects much shorter. His organs of generation were not very conspicuous, and seemed to be small in proportion to his size. The hair of his coat was smooth and glossy when he was first killed, and his teeth and appearance altogether indicated that he was young and in the full possession of his physical powers. Upon the whole," adds his biographer, "he was a wonderful beast to behold, and there was more in him to excite amazement than fear."

That this animal showed great tenacity of life is evident from his surviving so many dreadful wounds; and his peculiarity

liarity in this respect seems to have been a subject of intense surprise to all his assailants. In reference to this point it may be proper to remark, that after he had been carried on board ship, and was hauled up for the purpose of being skinned, the first stroke of the knife on the skin of the arm produced an instantaneous vibration of its muscles, followed by a convulsive contraction of the whole member. A like quivering of the muscles occurred when the knife was applied to the skin of the back, and so impressed Captain Cornfoot with a persuasion that the animal retained his sensibility, that he ordered the process of skinning to stop till the head had been removed.

It seems probable that this animal had travelled from some distance to the place where he was found, as his legs were covered with mud up to the knees, and he was considered as great a prodigy by the natives as by the Europeans. They had never before met with an animal like him, although they lived within two days journey of one of the vast and almost impenetrable forests of Sumatra. They seemed to think that his appearance accounted for many strange noises, resembling screams and shouts, and various sounds, which they could neither attribute to the roar of the tiger, nor to the voice of any other beast with which they were familiar. What capability the great Orang Outang may possess of uttering such sounds does not appear, but this belief of the Malays may lead to the capture of other individuals of his species, and to the discovery of more interesting particulars of his conformation and habits.

The only material discrepancy which I can detect in the different accounts which have been given of this animal, regards his height, which in some of them is vaguely stated at from above six feet to nearly eight. Captain Cornfoot however, who favoured me with a verbal description of the animal when brought on board his ship, stated that "he was a full head taller than any man on board, measuring seven feet in what might be called his ordinary standing posture, and eight feet when suspended for the purpose of being skinned."

The following measurements, which I have carefully made of different parts of the animal in the Society's Museum, go far to determine this point, and are entirely in favour of Captain Cornfoot's accuracy. The skin of the body of the animal dried and shrivelled as it is, measures in a straight line from the top of the shoulder to the part where the ancle has been removed, 5 feet 10 inches, the *perpendicular* length of the neck as it is in the preparation $3\frac{1}{2}$ inches, the length of the head from the top of the forehead to the end of the chin 9 inches, and the length of the skin still attached to the foot from its

line

line of separation from the leg 8 inches:—we thus obtain 7 feet 6½ inches as the approximate height of the animal. The natural bending posture of the ape tribe would obviously diminish the height of the standing posture in the living animal, and probably reduce it to Captain Cornfoot's measurement of 7 feet, whilst the stretching that would take place when the animal was extended for dissection might as obviously increase his length to 8 feet.

Description of the remains of the animal.

HEAD.—The face of this animal with the exception of the beard is nearly bare, a few straggling short downy hairs being alone scattered over it, and is of a dark lead colour. The eyes are small in relation to those of man, and are about an inch apart: the eyelids are well fringed with lashes. The ears are one inch and a half in length, and barely an inch in breadth, are closely applied to the head, and resemble those of man, with the exception of wanting the lower lobe. The nose is scarcely raised above the level of the face, and is chiefly distinguished by two nostrils three-fourths of an inch in breadth, placed obliquely side by side. The mouth projects considerably in a mammillary form, and its opening is very large; when closed, the lips appear narrow, but are in reality half an inch in thickness. The hair of the head is of a reddish brown, grows from behind forwards, and is five inches in length. The beard is handsome and appears to have been curly in the animal's life-time, and approaches to a chesnut colour; it is about three inches long, springing very gracefully from the upper lip near the angles of the mouth, in the form of mustachios, whence descending, it mixes with that of the chin, the whole having at present a very wavy aspect. The face of the animal is much wrinkled.

HANDS.—The palms of the hands are very long, are quite naked from the wrists, and are of the colour of the face. Their backs, to the last joint of the fingers, are covered with hair, which inclines a little backwards towards the wrists and then turns directly upwards. All the fingers have nails, which are strong, convex, and of a black colour; the thumb reaches to the first joint of the fore-finger.

FEET.—The feet are covered on the back with long brown hair to the last joint of the toes: the great toe is set on nearly at right angles to the foot, and is relatively very short. The original colour of the palms of the hands and the soles of the feet is somewhat uncertain, in consequence of the effect of the spirit in which they have been preserved.

 SKIN.

SKIN.—The skin itself is of a dark leaden colour. The hair is of a brownish red, but when observed at some distance, has a dull, and in some places an almost black appearance; but in a strong light it is of a light red. It is in all parts very long; on the fore-arm it is directed upwards; on the upper arm its general direction is downwards, but from its length it hangs shaggy below the arm; from the shoulders it hangs in large and long massy tufts, which in continuation with the long hair on the back, seem to form one long mass to the very centre of the body. About the flanks the hair is equally long, and in the living animal must have descended below the thighs and nates. On the limits, however, of the lateral termination of the skin which must have covered the chest and belly, it is scanty, and gives the impression that these parts must have been comparatively bare. Round the upper part of the back it is also much thinner than elsewhere, and small tufts at the junction of the skin with the neck are curled abruptly upwards, corresponding with the direction of the hair at the back of the head.

In the dimensions which I am about to give of the skin, I have stated that it measures from one extremity of the arm to another five feet eight inches; to this is to be added fifteen inches on each side for the hands and wrists, which will render the whole span of the animal equal to eight feet two inches.

The following are the measurements which I have made of the different parts:

FACE.

Length of the forehead from the commencement of the hair to a point between the eyes	4½
From between the eyes to the end of the nose	1½
From the end of the nose to the mouth	3
From the mouth to the setting on of the neck	4½
Circumference of the mouth	6

SKIN.

	ft.	*in.*
Greatest breadth about the centre of the skin	3	
Greatest length down the centre of the back	3	
Length from the extremity of one arm where it is separated from the wrist to the other	5	8
Breadth of the skin from the situation of the os coccygis to the setting on of the thigh	1	4
Across the middle of the thigh	1	0
Greatest length of the hair on the shoulders and back	0	10

MEASUREMENT OF HANDS AND FEET.

Front measurement of hand.

	ft.	*in.*
Length of hand from the end of the middle finger to the wrist in a right line	1	0
Circumference of hand over the knuckles	0	11
Length of palm from the wrist	0	6½

Length

	ft.	
Length of middle finger	0	5¼
——— of fore finger .	0	4¾
——— of little finger .	0	4¼
——— of ring finger .	0	5
——— of thumb . . .	0	2½

Back measurement of hand.

Length of ring finger . .	0	6¾
——— of middle finger .	0	6¾
——— of little finger . .	0	5¾
——— of fore finger . .	0	6
——— of thumb	0	4

Front measurement of feet.

Length from the end of the heel to the end of the middle toe		
——— of sole of the foot		9¾
——— of middle toe .		4¼
——— of ring toe . .		4¼
——— of little toe . .		3½
——— of fore toe . .		3¼
——— of great toe .		2¾
Circumference over the knuckles of the toes .	0	9¾

Back measurement.

Length of middle toe . .		6
——— of fore toe . . .		5½
——— of ring toe . . .		6
——— of little toe . . .		5
——— of great toe . . .		4½

Measurement of the lower jaw.

	ft.	
Circumference of the jaw round the chin	0	11½
Length of the ramus from the head of the jaw to its b[illegible]		
Breadth of the ramus or ascending portion of the jaw at a level with the teeth	0	2¼
Depth of the jaw at the symphysis menti . . .	0	2½

MEASUREMENT OF THE TEETH.

Number of teeth 32; namely, 2 Canine, 10 Grinders, and 4 Incisive Teeth in each jaw.

Canine Teeth.

Whole length of lower canine teeth	2·7
Greatest length of fang . .	·2
Smallest ditto	1·6
Greatest length of the enamel or exposed part of the teeth	1·1
Part exceeding the other teeth in length	·4
Lateral breadth measured on a level with the jaw .	·6
Breadth from before inwards	·7

Incisive Teeth.

Whole length of the lateral	1·5
Of enamel exposed . . .	·7
Breadth of cutting surface .	·4
Ditto of central teeth . .	·4

The front teeth of the upper jaw greatly resemble those of the lower, with the exception of the middle incisive teeth, which are twice the width of the lateral ones.

XLV. *Astronomical Observations* 1827. *By Lieut.* GEORGE BEAUFOY, *R. N.*

Bushey Heath, near Stanmore.

LATITUDE 51° 37′ 44″·3 North. Longitude west in time 1′ 20″·93.

Observed transits of the moon, and moon-culminating stars over the middle of the transit instrument in sidereal time.

1827.	Stars.	Transits.
Jan. 3.	18 λ Piscium	23° 33′ 14″·41
3.	19 Piscium	23 37 34·30
3.	Moon's First Limb.	23 42 45·05

 1827.

1827.	Stars.	Transits.		
Jan. 3.	28 ω Piscium	23°	50′	27″·35
5.	Moon's First Limb	1	19	50 ·01
5.	102 π Piscium	1	27	57 ·59
9.	α Tauri	4	26	02 ·73
9.	95 τ Tauri	4	31	54 ·99
9.	Moon's First Limb	4	40	51 ·96
9.	102 ι Tauri	4	52	48 ·25
10.	123 ζ Tauri	5	27	21 ·27
10.	Moon's First Limb	5	33	32 ·06
11.	Saturn's Centre	6	05	11 ·10
11.	13 μ Geminorum........	6	12	32 ·89
11.	18 ν Geminorum........	6	18	43 ·92
11.	Moon's First Limb......	6	26	31 ·39
11.	26 u Geminorum........	6	32	22 ·38
12.	54 λ Geminorum	7	08	11 ·26
12.	Moon's First Limb	7	19	17 ·04
12.	68 k Geminorum........	7	23	46 ·29
14.	65 α^2 Cancri	8	49	03 ·29
14.	76 κ Cancri............	8	58	24 ·31
14.	Moon's Second Limb....	9	04	57 ·13
14.	5 ξ Leonis	9	22	38 ·87
14.	14 o Leonis............	9	31	56 ·71
19.	7 δ Corvi	12	20	57 ·29
19.	67 α Virginis	13	16	06 ·77
19.	Moon (22)	13	23	52 ·36
Feb. 3.	Moon (8)	2	38	46 ·86
3.	57 δ Arietis............	3	01	47 ·31
4.	60 Tauri	3	17	13 ·42
4.	Moon (9)	3	29	17 ·89
4.	215 Tauri.............	3	50	52 ·96
6.	102 ι Tauri	4	52	48 ·45
6.	109 n Tauri............	5	08	55 ·90
6.	Moon (11)	5	12	54 ·14
6.	123 ζ Tauri...........	5	27	21 ·15
7.	Saturn	5	58	17 ·29
7.	Moon (12)	6	05	37 ·21
7.	18 ν Geminorum........	6	18	44 ·01
8.	24 γ Geminorum........	6	27	45 ·43
8.	43 ζ Geminorum........	6	53	53 ·22
8.	Moon (13)	6	58	27 ·71
8.	54 λ Geminorum........	7	08	11 ·21
9.	74 f Geminorum	7	29	31 ·45
9.	Moon (14)	7	51	05 ·20
9.	25 d^2 Cancri	8	16	04 ·32

Jan. 12th. Immersion of Jupiter's third satellite	14^h	12^m	14^s	M. T. at Bushey.
	14	13	35	M. T. at Greenwich.
Jan. 14th. Immersion of Jupiter's first satellite..........	16	04	37	M. T. at Bushey.
	16	05	58	M. T. at Greenwich.

Feb.

Feb. 2nd. Immersion of Jupiter's second satellite	12^h 17^m 09^s	M. T. at Bushey.
	12 18 30	M. T. at Greenwich.
Feb. 9th. Immersion of Jupiter's second satellite	2 51 00	M. T. at Bushey.
	2 52 21	M. T. at Greenwich.

These observations were made with one of Mr. Dollond's 5 feet achromatic telescopes,—the magnifying power 86.

Summary of a Meteorological Table, kept at Bushey Heath in 1826.

The Barometer, Thermometer, and Winds were observed between 9 and 10 o'clock in the morning, at which hour the temperature of the external air is nearly the same as the mean temperature. See column 3 and 8.

The *greatest* altitude of the mercury in the barometer was on December 28th, 30·068 inches; the *least*, on the 14th November, 28·590 inches. Thermometer highest 28th of June, 88°; lowest 16th January, 19°.

Months.	Barom.	Ther.	Rain.	Evap.	Six's Thermometer.			Winds.							
					Min.	Max.	Mean.	N.	NE.	E.	SE.	S.	SW.	W.	NW.
	Inches.		Inches.	Inches.	°	°	°								
Jan.	29·542	31·1	0·328	0·700	28·6	52·6	30·60	1	9	5	6	0	4	0	6
Feb.	29·504	41·3	1·990	1·58	38·1	47·2	42·65	0	0	0	5	5	14	3	1
March..	29·488	41·4	1·607	2 68	37·3	41·4	43·15	1	10	1	5	0	7	1	6
April ...	29 335	48·8	0·690	4·19	42·5	56·7	49·61	0	2	1	1	1	10	4	11
May	29·592	52·2	2·477	3·17	44·7	58·2	51·45	7	14	1	2	0	1	1	3
June ...	29·775	66·2	0 594	5·50	55·8	74·4	65·10	1	13	0	2	0	4	2	1
July ...	29·519	65·4	2·095	5·94	58·6	73·8	66·20	1	5	1	2	0	17	2	3
August.	29·526	64·3	2 073	6·46	58·8	72·6	65·70	2	5	0	3	1	13	1	5
Sept. ...	29·428	57·7	4·026	2·43	53·0	63·9	58·45	0	7	3	5	0	9	3	3
Oct.	29·437	52·5	2·221	1·46	49·4	57·4	53·40	0	3	1	8	0	11	4	4
Nov. ..	29·330	39·4	2·805	1·00	35·3	44·5	39·90	0	9	0	2	0	9	1	9
Dec. ...	29·406	41·12	1·930	0·97	38·9	45·0	41·95	0	7	2	2	3	8	0	9
Year.	29·490	50·12	22·836	36·08	45·01	55·56	50·68	13	84	15	43	10	107	22	61

XLVI. *On a New Mineral Species.* *By* A. Levy, *Esq. M.A. F.G.S.**

MR. HEULAND has lately added to his collection a small group of quartz slightly chlorited, upon which are seen some crystals belonging, I believe, to a new species, which at his suggestion I propose to call Mohsite, in honour of Professor Mohs.

An acute rhomboid of 73° 43′ represented fig. 1. may be considered as the primitive form of this substance. It does not yield to mechanical division in any direction, as far as I could judge upon the small quantity I had to examine. The fracture is conchoidal and shining. It is brittle, but scratches glass very easily. It is opaque, iron black, and possesses a high metallic lustre. It has not the least action on the magnet.

All the crystals upon the specimen I have seen are twin

* Communicated by the Author.

crystals,

crystals, flattened in a direction perpendicular to the axis of the primitive rhomboid, and present the aspect of small flat tables almost circular, with alternate re-entering and salient angles on their edges. The form of the individuals which compose these macles is represented by fig. 2: all the planes are very brilliant, except those marked d^1, d^2, which are less shining, but sufficiently so, however, to allow the use of the reflecting goniometer to measure their incidences. The angles are as follow:

$p, a^1 = 112° 30'$	$p, p = 73° 43'$	
$b^1\ a^1 = 129\ \ 39$	$b^1, b^1 = 96\ \ 22$	
$c^1, a^1 = 101\ \ 42$	$e^1, e^1 = 64\ \ 00$	
$P, d^2 = 157\ \ 10$	$d^2, d^2 = 142\ \ 14$	$d^2, d^2 = 99° 22'$

The manner in which the two individuals are grouped in the macles is very remarkable; their axes coincide, or are parallel; and to have their relative position it is necessary to suppose, that, being first in a parallel position, one of them has turned 30° or 90° round the axis, instead of 60° or 180° as is generally the case in the macles offered by crystals derived from a rhomboid. The thickness of the two crystals is the same, and their faces a^1 are on the same level, and form only one plane.

Another remarkable fact to be noticed with respect to this new substance, is its almost perfect isomorphism with Eudyalite. The primitive form of the last substance is an acute rhomboid of 73° 40′, differing only by 3′ of the primitive form of Mohsite: and moreover, out of the six modifications which compose the crystal just described,—five, P, a^1, c^1, b^1, d^1, occur on the variety of Eudyalite I have described in the Edinburgh Philosophical Journal for January 1825.

Fig. 1.

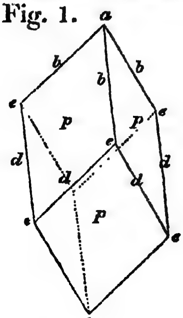
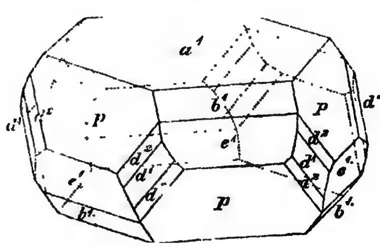


Fig. 2.



It seems from the appearance of the group of rock crystals upon which this substance occurs, that there can be no doubt that the specimen comes from Dauphiny. This circumstance, added to the analogy of some of the exterior characters, might suggest the idea that Crichtonite and Mohsite belong to the same

same species: and in support of this opinion, I find that a rhomboid measuring very nearly the same angle as the acute rhomboid of Crichtonite may be derived from the primitive adopted for Mohsite by the simple law $e^{\frac{4}{3}}$. But however, it may be observed, that a rhomboid so acute as that of Crichtonite may be derived by simple laws, from many rhomboids;—thus, for instance, that rhomboid is derivable by a still simpler law $e^{\frac{3}{2}}$ from the primitive form of specular iron, or of axotomous iron. Besides, Crichtonite presents a cleavage in a direction perpendicular to the axis, and is not sufficiently hard to scratch glass,—two characters which differ from those of Mohsite.

XLVII. *Notices respecting New Books.*

An Historical and Descriptive Account of the Steam-Engine, comprising a General View of the various Modes of employing Elastic Vapour as a prime Mover in Mechanics: with an Appendix of Patents and Parliamentary Papers connected with the Subject. By C. F. PARTINGTON, of the London Institution. Second Edition, Corrected and E‥larged. London, 1826. 8vo. pp. 300. Plates and Diagrams 33.

T been already so well appretiated by the public, that on the present occasion we need only point out the improvements it has received in this second edition. The only additional section it contains is an article on steam-boats, from the pen of Mr. Tredgold, furnishing some important mathematical data for the construction of the paddle-wheels; but several useful tables, and a variety of particulars respecting the progressive improvement and present state of the steam-engine in its different forms, have been incorporated in their proper places. A number of engravings on wood have also been added, representing on an enlarged scale some of the most important parts of the steam-engine, &c.; together with a quarto plate of a locomotive engine and sections of a steam-vessel. Some less important or redundant statements in the former edition have been omitted; and the entire work, we think, has been rendered more useful than before.

Geological and Historical Observations on the Eastern Valleys of Norfolk. By J. W. ROBBERDS, jun.

This interesting tract furnishes a pleasing instance how much assistance may be obtained, from studies which have apparently no mutual connexion, in the investigation of any branch of knowledge or subject of inquiry. Mr. Robberds has been led by an examination of the district which has been the object of his attention, to dispute the conclusion of Cuvier, De Luc, and others, that no alteration in the height of the waters of the ocean has taken place for many ages. "If," says the latter, "the depression of the level of these seas were a matter

a matter of certainty, the best authenticated and the least equivocal monuments of their change would abound along their coasts; but proofs are every where found that such a change is chimerical." "Yet," says Mr. Robberds, "the eastern valleys of Norfolk afford throughout the whole of their extent those clear traces of the former residence of the sea, which, M. De Luc here says, are not to be found in any such districts; and the gradual retreat of its waters is in this instance matter almost of positive historical record."

That the valleys in question were formerly branches of a wide æstuary occupied by the sea, Mr. Robberds endeavours to establish, 1st, by *physical proofs*, and in particular by the traces of a former beach composed of recent shells and loose sand, rising always to the same level of about forty feet above the river, following the course of these valleys and their recesses on its opposite sides, and not penetrating beyond the surface of the hills.

2ndly, By *historical proofs:* viz. tradition; remains of antiquity; etymology of names of villages, &c.; and positive records. With much learning, ingenuity and judgement, the author has brought forward a considerable body of evidence of this kind, strikingly corroborative of his physical proofs: among these are various Roman forts, which, though now some way in-land, yet were apparently built for the protection of the coasts; the incursion of Sweyn with his fleet to Norwich in 1004; the *salinæ* or salt works enumerated in Domesday Book as existing at various villages eight miles from the present coast; records which prove Yarmouth to have been an island in 1347; and law proceedings in 1327, which show that up to that time ships had come up to Norwich laden with merchandize: "all these," the author states, "concur to prove that the eastern valleys of Norfolk were formerly branches of a wide æstuary, and that their present rivers and lakes are the remains of that large body of water by which their surface was overspread, even in times comparatively recent;" and he concludes by inferring that the change "has been the result of a depression of the German Ocean itself."

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

IN consequence of the decease of H.R.H. the Duke of York, this Society did not resume its sittings until Jan. 25; when the name of Professor Jameson was ordered to be inserted in the printed lists of the Society: and a paper was read, entitled "On the expediency of assigning specific names to all such functions of simple elements as represent definite physical properties; with the suggestion of a new term in mechanics: illustrated by an investigation of the machine moved by recoil, and also by some observations on the steam-engine; by Davies Gilbert, Esq. M.P. V.P.R.S."

In the commencement of this paper, the author shows the utility of distinguishing by separate appellations all such functions as mea-

sure

sure the intensity of physical properties; adverting, in proof of this, to the acrimonious controversy which took place soon after the application of mathematical expression to the laws of motion; in which it was contended by some mathematicians, that the weight of a moving body multiplied into its velocity was the measure of the motion; whilst others of equal eminence maintained that the weight should be multiplied into the square of the velocity, in order to obtain the measure. But it was at length discovered that these views were not in reality at variance with each other, and the introduction of the terms *momentum* and *impetus* respectively applied to them, terminated the dispute. After referring to some observations on the subject by G. Attwood, Mr. Gilbert remarks, that in the Bakerian Lecture for 1806, by a Fellow of the Society [Dr. Wollaston], who has touched upon nothing that he has not elucidated and adorned, it is stated that neither of these terms is usually a correct measure of the effective action of machines. The criterion of this is the force exerted multiplied by the space through which it acts; and this measure, numerically expressed, and with reference to the steam-engine, has been denominated *Duty* by Mr. Watt, and the raising of one pound one foot high has been made by him the dynamic unit; according to which estimate the *Duty* performed by one bushel of coals of 84 pounds has been found to vary from 30 to 50 millions of such units, according to the nature of the engine and the mode of combustion employed. To the measure or function represented by the force multiplied by the space through which it acts, the author, however, proposes to give the name *efficiency;* retaining the word *duty* for a similar function indicative of the work performed; and by a comparison of these two functions; viz. the efficiency expended on, and the duty performed by, any machine, an exact measure of its intrinsic work will be obtained.

The author then proceeds to show the utility of his new term in investigating the mechanical value of the recoil-engine, and by an algebraic process, taking every thing most favourably to the engine, arrives at the conclusion that the duty cannot, even in the best state of its action, materially exceed half the efficiency, and that in consequence it can never be used with advantage. The water-wheel, and the pressure-engine offering much greater duties, while the wheel possesses the advantage of preserving a uniformity of efficiency during the whole action, which is not the case with the recoil-engine. And these considerations lead him to remark on the impossibility of carrying into effect a plan proposed by some eminent engineers for applying steam on a principle of recoil.

To estimate the efficiency of steam acting uniformly with its entire force, the author assumes from experiment, that a bushel of coals can convert into steam 14 cubic feet of water, occupying 1330 times that space in the state of steam, and therefore lifting an atmosphere incumbent on the surface of the water, uniformly to 1330 times its depth;—thus giving an efficiency of about 39 millions of pounds raised one foot high. From this he concludes that (all de-

ductions made) 30,000,000 would probably be the utmost attainable limits of duty but for two expedients; 1st, causing the steam to act expansively after exerting its whole force through a certain part of the cylinder; 2dly, raising its temperature by an increased expense of fuel much above 212°.

Both these means are considered, and occasion is taken to compare the efficiency of the methods invented by Messrs. Watt and Hornblower, for the former purpose; the preference in point of simplicity and advantage being given, however, to the former. With regard to the latter, it is concluded, that in certain cases advantage is really gained by the use of strong steam. The author then alludes with approbation to a method recently attempted, where a small quantity of water is forced at each stroke into a minute boiler, presenting, however, a very large surface in proportion to its capacity, and kept at an equable high temperature by immersion in fused metal. But he considers the greatest hopes of increased power to rest on the application to mechanical purposes of some fluid more elastic than the vapour of water, according to the suggestion of the President in the Phil. Trans. for 1823.

The author concludes this paper by a statement of the duties actually performed by the engines in Cornwall; from which it appears that several of the large engines there at work are actually performing a duty greater than the whole efficiency of the steam, unaided by expansive working or high pressure, on the assumptions here made, while others apparently similar in every respect fail of performing half that duty, and no satisfactory cause has been assigned for this important difference.

Feb. 1.—G. Poulett Scrope, Esq. was admitted a Fellow of the Society; and a paper was read, entitled "Account of a new genus of serpentiform sea-animals; by J. Harwood, M.D. F.L.S., Professor of Natural History at the Royal Institution: communicated by Daniel Moore, Esq. F.R.S."

In the introductory portion of this paper Dr. Harwood alludes to the notion entertained by the ancients, that the sea was peopled by monsters and animals of anomalous nature; and to the modern fabulous relations concerning the kraken and the sea-serpent. The last subject he mentions as having of late years attracted considerable attention and given rise to much exaggerated narrative. He then states his gratification at being enabled to present to the Royal Society an account of a new marine serpentiform animal, which he has recently examined.

This animal was taken up at sea, in latitude 62° N., and longitude 57° W., by Capt. Sawyer, of the ship Harmony, of Hull, whilst he was pursuing the bottle-nosed porpoise. It was found lying on the surface of the water, and was at first supposed to be an inflated seal-skin, as employed by the Esquimaux to attach to their harpoons, for the purpose of wearying out the larger aquatic animals by its buoyant power.

From its continued endeavours, apparently, to gorge a species of perch, of greater circumference than itself, it was in a very exhausted state; and made scarcely any efforts to resist its capture. The

The author had examined it as preserved in rum by Capt Sawyer. In several characters this animal strongly resembles the Ophidian reptiles, especially in the formation of the jaws, which, with the exception of the apparent want of interarticular bones, are truly serpentiform; and from this resemblance, after showing the dissimilitude of the animal from the genera to which it is nearest allied, in conjunction with the remarkable character afforded by the large sac with which it is provided, Dr. Harwood assigns to it the generic appellation of *Ophiognathus*, with the following generic character: *Corpus nudum, lubricum, colubriforme, compressum, sacco amplo abdominali.*

Giving the specific name of *ampullaceus* to this animal, the author proceeds to describe it in detail. The specimen examined is about four feet six inches in length, is very slender, and the tail has a filamentous termination occupying about 20 inches of the entire length of the animal; this begins at the termination of the dorsal fin, which, like all the other fins, is small. The colour is a purplish black, the filamentous portion of the tail being lighter than the rest. The sac extends from near the extremity of the snout about twenty inches down the body, and when partially inflated is about nine inches in circumference: its greatest width, including the slender body of the animal, is four inches. At the distance of a few inches from its termination is the rectum, the course of which and of the other digestive organs, owing to the tenuity of the sac, can readily be traced. The author compares this appendage of the *Ophiognathus* with similar organs in other animals of various classes, remarking that the nearest approach to it among fishes is to be found in the *Diodons* and *Tetraodons*, which possess a large abdominal sac, on inflating which they become secure from the attacks of their enemies, by the mechanical erection of the spines upon it. In the present animal, however, its only use appears to be that of a float.

The edges of the spiracula with which the *Ophiognathus* is provided, partially conceal three tufted branchiæ on each side of the head. It has a single row of teeth above and below; no teeth on the palatial bones, and is destitute of a tongue. The jaws are so long, and their articulation of such a nature, that their opening is wider then that of any other animal that the author is acquainted with; not excepting even the Rattlesnake.

The entire form of this animal indicates that it must possess great swiftness of motion in the waters.

This paper is illustrated by three drawings of the *Ophiognathus* and its several parts.

Feb. 8.—A. Melville, Esq., and T. J. Pettigrew, Esq., were respectively admitted Fellows of the Society; and a paper was read, entitled " An Examination into the Structure of the Cells of the Human Lungs, with a view to ascertain the office they perform in Respiration; by Sir E. Home, Bart. V.P.R.S. Illustrated by microscopical observations, by F. Bauer, Esq. F.R.S."

The author commences this paper by remarking, that the subject of respiration has hitherto been regarded as belonging rather to chemistry than to anatomy; but that he finds reason to believe that

process to be more simple than is imagined, and more within the reach of anatomical than chemical investigation. The present theory among chemists, he states, is, that respiration decarbonizes the blood, a volume of oxygen and nitrogen being received into the lungs at each inspiration, and returned measure for measure, the oxygen only being partly converted into carbonic acid; thus proving, as they suppose, that no part of the atmospheric air breathed is retained. This theory Sir E. Home considers as satisfactory, supposing it to be supported by the structure of the lungs themselves, and taking it for granted that the blood requires no other change for its purification. But when it was devised, no accurate examination of the cellular structure of the lungs had been set on foot; and it is the object of the present communication to explain their mechanism, and ascertain to what extent it is fitted for the processes this theory requires.

The author began the investigation of this subject by inquiring into the circulation of blood through the lungs; and by the assistance of Mr. Russel, of St. George's Hospital, he procured injections of their veins and arteries, capable of being rendered objects of microscopical examination by Mr. Bauer.

The first fact discovered, was, that though an injection introduced by the pulmonary artery was found to return by the trunks of the pulmonary veins; yet, when thrown in by the veins it does not return by the arteries. The next fact ascertained, was, that the distention of the air-cells produces an interruption between the arterial and the venous circulation, the blood being carried no further than the small arterial branches surrounding the air-cells. Sir Everard next proceeds to describe the air-cells and parts surrounding them, from drawings made by Mr. Bauer. The branches of the pulmonary artery accompanied by larger and more numerous branches of the pulmonary vein, are seen ramifying behind the internal membrane of each cell: the latter have valves at regular intervals, and there are numerous absorbents supplied also with valves. The injection was found to have stopped short of the termination of the artery, and the space beyond to be filled with gas.

The conclusions deduced by the author from this investigation are unfavourable to the received doctrine of simple decarbonization; conceiving the structure thus developed as better adapted to receive supplies from the atmosphere, and transmit them to the heart. He considers that the carbonic acid detected by Professor Brande in urine and perspirable matter, must have been formed in the blood circulating through the arteries, and have derived the oxygen in its carbonic acid from the lungs. He considers further, that the carbonic acid carried off in respiration is furnished from such venous blood as has acquired it during the process of 'igestion; having shown on a former occasion that soon after that process has commenced, the oxygen employed in it unites with carbon.

LINNÆAN SOCIETY.

Feb. 6.—Several vacancies were declared in the list of the Foreign members, and the names of various distinguished Foreign naturalists were

were proposed. A paper was read, entitled "Observations on the Tracheæ of Birds, with descriptions and representations of several not hitherto figured: by William Yarrell, Esq. F.L.S."—The extraordinary structures described by the author, are that of the crested Pintado of Africa (*Numida cristata*, Pall.), the Demoiselle, the Stanley Crane, the Black Swan of New Holland, and other swans, geese, and ducks. It was remarked that all birds with a complex structure of trachea have loud harsh voices, while the simple forms belong to the delightful song-birds. The paper concluded with an arrangement of the British species of the Duck family.

Feb. 20.—A Description, by Bracy Clark, Esq. F.L.S. of a new species of Bot from the Illinois, was read. Also a continuation of Mr. W. S. MacLeay's paper on the Comparative Anatomy of certain birds of Cuba.

GEOLOGICAL SOCIETY.

Feb. 2.—The reading of a paper was concluded, "On the coal-field of Brora in Sutherlandshire, and some other stratified deposits in the north of Scotland;" by R. I. Murchison, Esq. Sec. G.S. F.R.S.

The Brora coal-field forms a part of the deposits, which on the S.E. coast of Sutherlandshire occupy a tract of about twenty miles in length, from Golspie to the Ord of Caithness; and three miles in its greatest breadth,—divided into the valleys of Brora, Loth, and Navidale, by the successive advance to the coast of portions of the adjoining mountain range which bounds them on the W. and N.W. The first of these valleys is flanked on the S.W. by hills of red conglomerate; which pass inland on the N.E. of Loch Brora, and give place to an unstratified granitic rock that forms the remainder, of the mountainous boundary.

With a view to the comparison of the strata at Brora with those of England, the author had previously examined the N.E. coast of Yorkshire, from Filey-Bridge to Whitby, comprising the coal-field of the Eastern Moorlands above the lias.

The highest beds at Brora consist of a white quartzose sandstone, partially overlaid by a fissile limestone, containing many fossils,—the greater number of which have been identified with those of the calcareous grit beneath the coral rag;—and along with these Mr. Sowerby has discovered several new species. The next beds, in a descending order, are obscured, in the interior, by the diluvium which is generally spread over the surface of these valleys, but are exposed on other places on the coast; and they consist of shale with the fossils of the Oxford clay, overlying a limestone resembling cornbrash and forest-marble, the latter associated with calciferous grit. To these succeed other sandstones, and shales containing belemnites and ammonites, through which the shaft of the present coal-pit is sunk, to the depth of near 80 yards below the level of the river Brora. The principal bed of coal is 3 feet 5 inches in thickness, and the roof is a sandy calcareous mixture of fossil shells, and a compressed assemblage of leaves and stems of plants, passing into the coal itself. The fossils of this and the superior beds are identical for the greater part, with those which occur in the strata above the

the coal in the E. of Yorkshire: and of the whole number of species collected by the author, amounting nearly to fifty, two-thirds are well known fossils of the oolite;—the remainder belonging to new species represented in the last numbers of the Mineral Conchology. The plant of which the Brora coal appears to have been formed, is identical with one of the most characteristic vegetables of the Yorkshire coast, but differs essentially from any of the plants found in the coal measures beneath the new red-sandstone:—It has been formed into a new genus by Mr. Konig, and is described by him in the present memoir, under the name of *Oncylogonatum.*

The author, therefore, considers the Brora coal, from its associated shells and plants, as the equivalent of that of the Eastern Moorlands of Yorkshire.

At Loth, Helmsdale, and Navidale, shale and sandstone overlie calcareous strata resembling the cornbrash and forest-marble, and these are in many cases dislocated where they are in contact with the granitic rock, and distorted where they approach it.

The base of the entire series above mentioned is seen at low water on the coast near the north and south Sutors of Cromarty, where the lias with some of its characteristic fossils is observable resting upon the sandstone of the red conglomerate,—the latter in contact with granitic rock.

On the N.W. coast of Scotland, several members of the oolitic series with their peculiar organic remains were recognized by the author in the isles of Skye, Pabba, Scalpa, Mull, &c.; where their occurrence was first noticed generally by Dr. MacCulloch.

A short sketch is given of the geognostic relations of the schists and sandstones of Caithness, which are probably referrible to the new red-sandstone;—some of these beds resembling the copper slate of Thuringia, and its associates: whilst the fossil fish recently discovered at Banniskirk, though the species is new, appear to belong to the same family with those of Mansfeldt, in Germany.

The paper concludes by adverting to the support given by the preceding facts to the great importance of zoological evidence in the identification of distant deposits:—since the existence in the N. of Scotland, of a large portion of the oolitic series of England, has been demonstrated from the agreement of organic remains, although the mineralogical characters of the beds containing these fossils are perfectly distinct at the extremes of the tract through which the strata are distributed.

HORTICULTURAL SOCIETY.

Jan. 2.—The following papers were read: Upon grafting the pear upon quince stocks; by Mr. Thomas Torbron, F.H.S.—An account and description of the different varieties of raspberries which have been cultivated and examined in the Garden of the Society; by Mr. William Sanderson.—An arrangement and description of the varieties of gooseberries cultivated in the Garden of the Society; by Mr. Robert Thompson.—A fine exhibition of flowers, and some excellent new Flemish pears, ripened in the garden of Andrew Arcedeckne, Esq of Glevering Hall in Suffolk, were placed upon the table. — The most remarkable vegetable, was the *Topinambour*

Jaune, a new French variety of the Jerusalem artichoke, which was stated to possess considerable merit.

ROYAL INSTITUTION OF GREAT BRITAIN.

The evening meetings of the members of this Institution commenced on Friday Jan. 26th, when Mr. Faraday gave a kind of experimental report on the late advances in magnetism, dependent on the discovery of M. Arago. This philosopher had found that when metals not magnetic in the ordinary sense of the word, *i. e.* exerting no action upon the magnetic needle when merely placed in its vicinity, were made to assume a state of motion, striking effects took place, which effects, upon further examination, were found to depend not upon the absolute motion of the metal, but upon the relative change of place of the metal and the needle. Thus a plate of copper made to revolve under a magnet had the power of drawing it 80° or 90° from its natural position; or if the magnet were made to revolve under the plate, it produced rotation of the latter. Again, if magnets and copper plates were made to vibrate, and copper plates or magnets in a state of rest brought near to them, the vibrations of the former were rapidly diminished, and soon ceased altogether. Experiments by Messrs. Babbage, Herschel, Christie, Nobili and others, were then referred to, and the general conclusion stated, that the magnet had the power of inducing magnetism in the approximated metal, but requiring time for the purpose of producing the phænomena in question. The powerful objections to this theory lately advanced by M. Arago, were then noticed; namely, that the induction which should equally take place when the body is at rest, and show itself by its power in attracting the pole, is not so indicated; and that upon close examination, the power, of whatever kind it may be, is a repulsive one.

On the tables in the Library were an ornamental lamp recently constructed by Mr. Bartholomew; specimens of dried plants prepared at Massachusetts, by the sect of people denominated Shaking Quakers; and numerous books, presented to the Library of the Institution.

XLIX. *Intelligence and Miscellaneous Articles.*

BROMINE.

M. JUST. LIEBIG procured this substance by M. Balard's process from the mother water of the salt-works of Theororshalle near Kreutznach. Thirty pounds of the water yielded nearly twenty grammes (about 308 grains) of bromine. M. Liebig repeated and confirmed many of M. Balard's experiments, and no phænomena appeared unfavourable to the opinions adopted by M. Balard as to the elementary nature of bromine. The following experiments were also made by M. Liebig: a spiral iron wire was heated to redness in a glass tube, and the vapour of bromine, which had been well dried by chloride of calcium, was passed over it. As soon as the bromine came into contact with the iron it became white hot, and fused without evolving any gaseous matter. The fused mass was of a bright yellow colour, resembling Naples-yellow; its structure was lamellar

lamellar and crystalline; it dissolved readily in water without imparting colour to it. The solution precipitated nitrate of silver of a bright yellow colour, and by chlorine bromine was extricated from it; it was protobromide of iron.

In another experiment a platina wire was substituted for an iron one; but this metal was not acted on, and the bromine lost none of its properties: lamp-black under the same circumstances did not act upon bromine.

By putting water and bromine into contact with iron filings, proto- or perbromide of iron are formed according to the proportions, and the mass becomes very hot.

Very pure bromide of potassium may be obtained by pouring a solution of potash into one of bromine in alcohol until the alcohol begins to be discoloured; this salt, evaporated to dryness and heated to redness, becomes black.

Bromide of silver dissolves readily in ammonia; after some time white brilliant crystals are deposited, which evolve ammonia when heated, and leave bromide of silver.

2.521 grammes of bromide of potassium gave by decomposition with nitrate of silver, 4.041 gr. of bromide of silver; which gives 94.11 for the atom of bromine, oxygen being 10.—*Ann. de Chim. et de Phys.* Nov. 1826.

COMPOUND NATURE OF BROMINE.

In opposition to the above-stated opinion as to the elementary nature of bromine, M. Chevreul announced to the Academy of Sciences on the 10th of October, that M. Dumas had discovered a chloride of iodine which had all the properties of bromine.—*Ferussac's Bulletin*, Dec. 1826.

ACTION OF THE ALKALINE CHLORIDES AS DISINFECTING SUBSTANCES.

M. Labarraque having stated it as his opinion that the chloride which he prepares as a disinfecting substance is quickly converted by exposure to the air into a muriate, M. Gaulthier de Claubry has made some experiments to determine the changes which actually occur.

Some well-saturated chloride of lime was dissolved in water and subjected to the action of a current of carbonic acid gas; in a very short time chlorine was evolved, and by continuing the operation for three hours, a gramme (15·4 grs.) of the chloride was completely converted into carbonate of lime, which did not contain a trace of chlorine. It is difficult to obtain chloride of lime quite free from muriatic acid, but the quantity was perfectly the same after as before the action of the carbonic acid.

A solution of chloride of lime was exposed to the air from the 13th of August till the 10th of October; it then contained no chlorine, and a precipitate of carbonate of lime was obtained. Chloride of soda is decomposed by carbonic acid like chloride of lime, but more slowly, because it does not form an insoluble salt. It is easy to explain why chlorides are preferable as disinfecting substances to the fumigations of chlorine; the carbonic acid derived from the decomposition

position of the animal matter, and contained in the air, expels the chlorine from its combinations; and as it acts slowly, the chlorine is less capable of acting upon the animal œconomy, but readily decomposes putrid miasmata; it is in fact a true fumigation of chlorine, only it is not strong, and much longer continued.—*Annales de Chim. et de Phys.* Nov. 1826.

MR. LESLIE'S INSTRUMENT FOR ASCERTAINING THE SPECIFIC GRAVITY OF POWDERS.

In the Annals of Philosophy for April 1826, an account was given of the above-mentioned instrument. The following remarks upon it are copied from Ferussac's *Bulletin*, Dec. 1826.

In the *Bulletin des Sciences, &c.* for September 1826, it will be seen that Mr. Leslie had recently contrived an instrument for measuring the density of powders. The description of this instrument agrees perfectly with that of the stereometer invented twenty-nine years since by Mr. H. Say, a highly distinguished French engineer, who unfortunately fell in Egypt: it is proper to inform Mr. Leslie that he has committed an error in claiming the honour of this discovery, which he will find recorded, with the drawings and complete descriptions, in the 23rd volume of the *Annales de Chimie*, 1797, p. 1. In addition to this, the instrument has been made and frequently used to ascertain densities, especially those of gunpowder, and it still exists in the *Ecole Polytechnique.*

PRODUCE OF COPPER MINES IN CORNWALL,

Sold at the public ticketings.

For Six Months ending 31st Dec. 1825.

Names and returns of eight principal Mines.

	Tons of Ores.	Tons of Copper.	Value of Ores. £	s.	d.
Consolidated and United Mines....	6710	641	67,040	0	0
East Crinnis	3848	281	27,506	11	6
Dolcoath	3865	250	25,456	11	0
Wheal Buller and Beauchamp	3547	228	23,108	13	6
Pembroke	3673	223	21,419	4	0
Poldice and Wheal Unity	2177	171	17,818	12	0
Lanescot	2140	166	15,794	14	0
Wheal Montague & Wheal Harmony	1182	165	18,040	4	6
	27142	2125	216,184	10	6
64 smaller mines.	29887	2236	224,160	8	6
Total....	57029	4361	440,344	19	0

Average produce per cent of the ores .. $7\frac{5}{8}$
Average standard of copper £137

 For

For Six Months ending 30th June 1826.

	Tons of Ores.	Tons of Copper.	Value of Ores.		
			£	s.	d.
Consolidated and United Mines....	8251	724	55,793	5	6
East Crinnis	4017	310	21,813	13	0
Pembroke	3652	242	16,985	10	0
Dolcoath	3617	230	17,282	14	6
Wheal Buller and Beauchamp.....	2946	199	15,284	2	6
Poldice and Wheal Unity..........	2148	190	14,961	19	6
Lanescot	2219	177	11,353	0	0
Ting-Tang.	1632	157	12,346	18	6
	28782	2229	165,821	3	6
63 smaller mines	31497	2436	182,805	13	1
Total	60279	4665	348,626	16	7

Average produce per cent of the ores . $7\frac{3}{4}$
Average standard of copper......... £110.

For Six Months ending 31st Dec. 1826.

	Tons of Ores.	Tons of Copper.	Value of Ores.		
			£	s.	d.
Consolidated and United Mines ...	7101	644	47,013	9	6
East Crinnis	3947	310	20,223	16	6
Dolcoath	4377	291	20,265	5	0
Pembroke	3481	253	16,885	15	0
Penstruthal	2824	234	16,830	12	0
Wheal Maiden and Carharrack	2313	208	15,245	10	6
Poldice and Wheal Unity..........	2137	193	14,110	5	6
Ting-Tang..........	1907	181	13,426	17	6
	28087	2314	164,031	11	6
57 smaller mines.....	34475	2787	195,589	14	0
Total......	62562	5101	359,621	5	6

Average produce per cent of the ores...... $8\frac{1}{8}$
Average standard of copper .... £104.

LECTURES.

LECTURES.

A Course of Lectures on the Sources and Nature of Terrestrial Heat and Light will be commenced at the Russel Institution, on the 5th of March; by E. W. Brayley, jun. A.L.S. This course will comprise experimental illustrations of the phænomena of combustion, incandescense, phosphorescence, the evolution of heat and light by common and voltaic electricity, and luminous animals.

ACCOUNT OF STEAM-ENGINES IN CORNWALL.

Extracted from the Monthly Account for December 1826.

The whole number of engines reported in the month was,

53 Pumping Engines, 14 Whim Engines, 3 Stamping Engines.

Of the pumping engines 50 are single and 3 double power, and 3 of the single power engines have combined cylinders; the diameters of the cylinders as under:

Engines.	Inch.			Bushels.
4	90	of coals consumed	in the month	11864
4	80	do.	do.	10332
1	76	do.	do.	2660
10	70	do.	do.	16078
1	64	do.	do.	1368
3	63	do.	do.	9507
7	60	do.	do.	11793
3	58	do.	do.	5899
3	53	do.	do.	5111
2	50	do.	do.	1044
1	48	do.	do.	1083
3	45	do.	do.	4498
1	42	do.	do.	1010
2	40	do.	do.	630
4	36	do.	do.	2619
2	30	do.	do.	1310
1	28	do.	do.	1080
1	27	do.	do.	1080
53				88971

Relative duty of the pumping engines expressed by the number of millions of pounds of water lifted one foot by each bushel of coal.

Greatest duty 47	millions nearly	by 1	engine	60		inch cylinder.
41	do.	by 1	do.	80		do.
40	do.	by 1	do.	90		do.
39	do.	by 1	do.	76		do.
38	do.	by 2	engines	63 & 70		do.
26	do.	by 3	do.	2 of 80 & 1 of 63		do.
36 to 34	do.	by 4	do.			
34 to 30	do.	by 5	do.			
under 30	do.	by 28	do.			
duty not reported						
		53				

Six Engines performimg best duty.

Mines.	Diameter of cylinder.	Load per square inch on the piston.	Length of the stroke in the cylinder.	No. of lifts.	Depth.		Diameter of the pump.	Time.	Consumption of coals in bushels.	Number of strokes.	Length of stroke in the pump.	Load in pounds.	Pounds lifted one foot high, by consuming one bushel of coals.	No. of strokes per minute.	Remarks, and Engineers' Names.
		lbs.	ft. in.		fath.	ft.	inches.				ft. in.				
Wheal Hope	60 inch single	8·37	9 0	1 1 1	46 11 11	5 2 2	15 12⅓ 11	Nov. 27th to Dec. 30th.	1242	261,890	8 0	27,766	46,838,246	5·5	Drawing all the load perpendicularly. Main beam over the cylinder. One balance-bob at surface.—*Grose.*
Wheal Vor	80 inch single	13·37	10 0	5 4 1 1	135 44 12 11	2 0 0 5	15 16 9½ 9¼	Dec. 4th to Dec. 29th.	3274	199,960	7 6	89,607	41,045,698	5·56	Drawing perpendicularly 135 fathoms, and on the underlay 27 fathoms. Main beam over the cylinder. Two balance-bobs under ground.—*Sims & Richards.*
Consolidated Mines	90 inch single	9·42	9 11	1 1 6	6 15 144	0 0 1	12 12 16	Dec. 7th to Jan. 2nd.	4680	304,500	7 6	81,673	39,854,853	8·12	Drawing perpendicularly with main beam over the cylinder. One balance-bob at surface.—*Woolf.*
Dolcoath	70 inch single	10·05	8 9	1 5 1 3 1	2 93 22 65 15	0 1 0 1 3	8½ 11½ 12 11¼ 13	Nov. 29th to Dec. 28th.	2660	264,970	7 3	55,021	39,375,762	6·3	Drawing perpendicularly 179 fathoms, and on the underlay 33 fathoms. Main beam over the cylinder. Four balance-bobs under ground, and one at the surface. 60 fathoms of dry rods in the shaft.—*Jeffrees.*
Ting-Tang	63 inch single	13·4	7 9	2 4 1 1 1	39 81 20 11 12	0 3 3 3 0	9 14 12 9 8	Dec. 7th to Jan. 2nd.	1980	229,520	6 9	48,646	38,063,288	6·1	Drawing perpendicularly, with main beam under the cylinder and 15 fathoms of horizontal rods under ground.—*Sims & Sons.*
Binner Down	70 inch single	6·12	10 0	1 1 1	2 23 40	5 2 4	10 9 18	Nov. 28th to Jan. 1st.	2628	420,550	7 6	31,395	37,680,271	8·6	Drawing all the load perpendicularly. Main beam over the cylinder.—*Thomas.*

It may be remarked, that the time of the year is the most unfavourable to the work of the engines; as from the abundance of water in the mines, many are pushed beyond their most advantageous rate.

Duty of the best whim engines, rotatory, double, 6 millions drawn 1 foot high, by each bushel of coals, or 30 kibbles, drawn from the depth of 100 fathoms by ditto.

Duty of the best stamping engines, rotatory, double, 15 millions lifted 1 foot high, by each bushel of coals.

Note.—The monthly report of the duty of steam engines in Cornwall, is taken and computed by Messrs. John and Thomas Lean, who are specially appointed and paid for that purpose by the adventurers in the mines, whose object is to obtain a correct comparative statement, by which they may ascertain the merits of the respective engines, and may judge of the skill and care of the engineers they employ.

Messrs. Leans have the custody of the keys of the counters on the engines, they themselves measure the capacity and lengths of the pumps, and they receive the returns of the quantity of coals consumed from the persons who measure it, and make oath of the consumption at the custom-houses for the debenture which is allowed.

The engineers whose names are given are not manufacturers of engines, nor are they allowed to participate in any business of that kind; they plan the construction and superintend the execution and erection of engines, for which they are paid according to the power of each; and they have the care of them after being erected, and direct repairs, &c. for which they receive regular salaries from the mines.

The manufacturers who principally make engines for the mines in Cornwall are: Messrs. Trevenan, Carne, and Wood, Copper-House Foundry, Hayle, Cornwall; Messrs. Harvey and Co., Hayle Foundry, Ditto; Messrs. Price and Co. Neath Abbey Iron-works, South Wales; and Messrs. Fox and Co. Perran Foundry, Cornwall.

NEW PATENTS.

To Robert Barlow, of Jubillee-place, Chelsea, for a new combination of machinery, or new motion for superseding the necessity of the ordinary crank in steam-engines, and for other purposes where power is required.—Dated the 1st of February 1827.—6 months allowed to enrol specification.

To John Frederick Daniell, esquire, of Gower-street, Bedford-square, for improvements in the manufacture of gas.—1st of February.—6 months.

To John Oldham, of Dublin, for improvements in the construction of wheels for driving machinery impelled by water or wind, also applicable to propelling boats, &c.—1st of February.—6 mon.

To Ralph Hindmarsh, of Newcastle-upon-Tyne, master mariner, for an improvement in the construction of capstans and windlasses. —1st of February.—6 months.

To

To Robert Stirling Clerk, minister of Galston, in Ayrdeyre, and James Stirling, engineer, of Glasgow, for improvements in air engines for moving of machinery.—1st of February.—6 months.

To John White, of Southampton, engineer and iron-founder, for improvements in the construction of pistons or buckets for pumps.—1st of February.—6 months.

To Samuel Parker, Argyle-place, Argyle-street, Westminster, bronzist, for improvements in the construction of lamps.—1st of February.—2 months.

To Antoine Adolphe Marcellin Marbot, of No. 38, Norfolk-street, Strand, for improved machinery for working or cutting wood into all kinds of mouldings, rebates, cornices, or any sort of fluted work.—3d of February.—6 months.

METEOROLOGICAL OBSERVATIONS FOR JANUARY 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·26 Jan. 26. Wind N.—Min. 29·29 Jan. 12. Wind NW.
Range of the mercury 0·97.

Mean barometrical pressure for the month	29·828
——— for the lunar period ending the 27th instant	29·869
——— for 15 days with the Moon in North declination . .	29·857
——— for 14 days with the Moon in South declination . .	29·881
Spaces described by the rising and falling of the mercury . . .	7·520

Greatest variation in 24 hours 0·760.—Number of changes 21.

Therm. Max. 54° Jan. 8th. Wind W.—Min. 20° Jan. 22. Wind N.E.
Range 34°.—Mean temp. of exter. air 38°·90. For 30 days with ☉ in ♑ 41·25
Max. var. in 24 hours 18°·00—Mean temp. of spring water at 8 A.M. 50°·62

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 7th	100°
Greatest dryness of the air in the afternoon of the 20th . . .	48
Range of the index	52
Mean at 2 P.M. 67°·7—Mean at 8 A.M. 74·1—Mean at 8 P.M.	74·0
——— of three observations each day at 8, 2, and 8 o'clock . .	71·9

Evaporation for the month 0·70 inch.
Rain near ground 1·000 inches.—Rain 23 feet high 0·935 inches.

Summary of the Weather.

A clear sky, 4; fine, with various modifications of clouds, 11; an overcast sky without rain, 11; foggy, ½; rain, snow & sleet 4½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
18	9	30	0	15	15	19

Scale of the prevailing Winds

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
5	6½	½	1	1	3½	7½	6	31

General Observations.—This month has been very variable, it having shown many varieties of weather: the first part was generally cold and humid, accompanied with light rains, hail, snow, sleet, and strong gales of wind; and the latter part was dry, frosty, and more calm, which made it seasonable, especially as the snow lay on the ground, &c. for eight days. The

The whole depth of snow here did not exceed two and a half inches, although the atmosphere was apparently loaded with it, and more or less fell on *ten* different days; but in the northern districts it is reported to have been many feet deep. On the morning of the 4th instant the thermometer receded to 22 degrees; and the *maximum* temperature of the air on the 8th was 54 degrees! From the 6th to the 18th the weather in general was wet and mild; but from the 19th to the 27th it got cold again by the induction of a North-east wind, and in the morning of the 23rd, the thermometer sunk to 20 degrees; consequently, the water in the pumps was frozen, also the moats round the fortifications, and the ponds and marshes, which afforded skaiting for a few days. A change of wind to the Southwest on the 28th broke up the frost, a gradual thaw commenced, and on the 31st the thermometer reached to 52 degrees. The *maximum* temperature of the 4th, 6th, 13th, and 16th occurred in the nights instead of in the days. The mean temperature of the external air this month, is 1·17 degree under the mean of January for the last ten years. The atmospheric and meteoric *phænomena* that have come within our observations this month, are one paraselene on the north side of the moon in the evening of the 16th, one large lunar halo in the evening of the 11th, three meteors at 10 o'clock in the evening of the 18th, when the Aurora Borealis was in its greatest spendour; and ten gales of wind, or days on which they have prevailed, namely, three from the N., one from N.E., two from S.W., one from W., and three from the N.W.

REMARKS.

London.—First month. 1. Fine. 2. Fine: a little snow. 3. Fine: a little snow. 4. A little snow during the night, day fine. 5. Some snow early this morning. 6. Cloudy: some hail P.M. 7. Cloudy, drizzly. 8. Cloudy. 9. Cloudy and fine. 10. Rainy. 11. Snow and sleet during the day. 12. Cloudy. 13. White frost: drizzly: rain. 14. Wind very boisterous all day, with rain at intervals. 15. Very clear morning: fine day. 16—18. Cloudy. 19. Some snow in the afternoon. 20. Hoar frost: fine day. 21. Snowy day. 22. Snow with driving wind from S.E.: ground deeply covered with snow. 23. Snow showers. 24. Snowy. 25. Fine. 26. Hoar frost and foggy morning. 27. Cloudy and fine. 28. Fine: thaw commenced about 11 A.M. 29—31. Cloudy.

RESULTS.

Winds, N. 1: N.E. 2: E. 1: S.E. 2: S. 3: S.W. 5: W. 5: N.W. 12.
Barometer mean height for the month .... 30·063 inch.
Thermometer, mean height for the month.................. 34·145°
Evaporation .... ·78 inch.
Rain .... 1·15 inch.

Boston.—Jan. 1, 2. Cloudy. 3. Fine: snow A.M. 4. Fine: snow P.M. 5, 6. Cloudy. 7. Cloudy: rain A.M. 8. Cloudy. 9. Cloudy: violent storm, with rain P.M. 10. Cloudy; storm with rain P.M. 11. Stormy. 12. Cloudy. 13. Cloudy: rain A.M. 14. Stormy. 15. Fine. 16. Cloudy: rain 3 o'clock A.M. 17. Cloudy. 18. Cloudy: rain A.M. 19, 20. Cloudy. 21. Snow. 22. Stormy. 23. Snow. 24. Cloudy: heavy fall of snow in the night: snow now knee-deep on the level. 25. Fine. 26. Cloudy: Snow at intervals day and night. 27. Fine. 28. Cloudy: rain P.M. 29—31. Cloudy.

Penzance.—Jan. 1. Showers: fair, rain at night. 2. Fair: at times clear. 3. In general clear. 4. A fall of snow. 5. Cloudy. 6—8. Misty. 9. In general fair. 10. Heavy showers. 11, 12. Showers. 13. Misty. 14. Showers. 15. Fine day. 16. Showers. 17, 18. Misty rain. 19. Misty. 20. Fair. 21. Snow: clear ice. 22. Clear: fair. 23. Clear. 24. Cloudy. 25. Cloudy: snow. 26. Fair: hail shower. 27. Clear. 28. Cloudy. 29—31. Misty.

Meteor-

[Meteorolog]ical Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. [Veall at Boston].

Days of Month, 1827.		Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Gosport. Max.	Barometer. Gosport. Min.	Barometer. Boston 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Gosport. Max.	Thermometer. Gosport. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Penz.	Wind. Gosp.	Wind. Bost.	Evapor. Lond.	Evapor. Gosp.	Rain. Lond.	Rain. Penz.	Rain. Gosp.	Rain. Bost.
Jan.	1	30 16	29 75	29 90	29 70	29 98	29·68	29 50	48	34	51	46	51	37	42 5	W.	W.	W.	W.	...	...	·24	...	0·290	...
	2	29 75	29 74	29 54	29 52	29·57	29 51	29 22	38	21	45	40	42	27	34	NW.	NW.	W.	W.	...	...	...	...	·020	...
	3	29 87	29 74	29 55	29 50	29 45	29 43	29 35	26	12	40	31	34	22	18	NW.	NE.	NE.	W.	...	0 10	..	0·270	...	...
	4	30 29	29·87	29 85	29 65	29 70	29 62	29 53	30	12	36	24	32	29	12·0	NW.	NE	N.	W.	...	...	...	...	·010	...
☽	5	30 52	30 29	30 10	30 00	30 22	29 97	29 96	32	22	40	29	35	28	21 5	NW.	NE.	N.	NW.	...	...	...	...	...	...
	6	30 52	30 35	30 10	30 00	30 26	30 20	30 11	41	29	49	29	44	40	29	SW.	W.	N.	calm	...	·10	...	...	·085	...
	7	30 35	30 18	29 98	29 98	30 10	30 09	29 90	48	47	51	32	49	45	38 5	SW	W.	W	calm	...	...	09	...	...	·30
	8	30 18	30 05	29 98	29 90	30 04	29 94	29 66	50	42	51	47	54	44	50	SW.	W.	W.	W.	..	...	...	1·350	·020	...
	9	30 07	30 05	29 90	29 88	29 90	29 88	29 47	46	40	50	45	50	41	40 5	NW.	NW	W.	W.	...	05	...	...	...	...
	10	30 05	29 52	29 65	29 50	29 90	29 44	29 53	52	37	54	45	51	41	40 5	W.	SW.	SW.	calm	...	...	·40	...	·190	·05
	11	29 52	29 45	29 50	29 38	29 37	29 34	28 97	50	32	51	42	46	36	38 5	W.	NW.	W.	W.	...	...	08	0·330	·150	·20
	12	30 15	29 46	29 70	29 67	29 83	29 29	29 03	44	28	48	35	44	32	35	NW.	W.	NW.	calm	...	05	...	0 225	·010	..
○	13	30 15	29 58	29 80	29 79	29 94	29 77	29 70	50	43	54	46	50	46	34	W.	W.	W.	calm	...	..	...	..	·030	12
	14	30 34	29 58	29 60	29 60	29 70	29 44	28 95	40	21	54	48	51	33	52	W	W.	W.	W.	.	...	04	...	·030	...
	15	30 41	30 24	30 10	30 00	30 25	30 12	29 00	40	33	50	36	46	42	35	NW.	NW.	NW	W.	...	·10	...	0·175	...	..
	16	30 42	30 23	30 10	30 08	30 09	30 02	29 72	48	33	50	40	52	42	42	NW	W.	W.	SW.	...	...	...	...	·030	08
	17	30 44	30 42	30 04	30 02	30 22	30 17	30 00	43	31	51	48	45	35	36 5	NW	NW	NE	calm	...	...	...	0 260	·045	...
	18	30 45	30 41	30 00	30 00	30 20	30 16	30 05	40	24	50	48	43	35	35 5	E	NW	NE	calm	48	10	...	...	...	·07
	19	30 46	30 41	30 00	29 98	30 23	30 19	30 10	36	20	47	45	40	29	33	N.	NE.	NE.	calm	..	...	...	...	...	...
☾	20	30 41	30 10	30 00	29 86	30 17	30 04	30 05	35	23	42	38	36	28	31	NW	SE.	NE.	calm	...	...	...	...	...	...
	21	30 10	29 75	29 70	29 60	29 78	29 60	29 80	30	20	40	32	32	23	28	NE.	E.	NE	calm	...	05	...	...	·020	...
	22	29 83	29 75	29 40	29.40	29 56	29 38	29 50	30	13	40	25	33	20	30	SE.	E.	NE	E.	...	..	...	...	040	...
	23	29 83	29 79	29 50	29 50	29 57	29 55	29 45	32	26	34	21	34	26	22 5	NW	E.	N	calm	...	...	..	0 400	...	...
	24	29 85	29 83	29 49	29 48	29 57	29 52	29 40	35	23	40	25 5	42	32	24 5	S.	NW.	W	calm	...	05	...	...	...	...
	25	29 89	29 85	29 40	29 35	29·58	29 57	29 58	36	13	40	30	40	23	16 5	NE	W.	NE.	calm	...	...	...	...	·020	...
	26	30 32	29 89	29 70	29 55	29 76	29 65	29 55	31	24	38	28 5	38	32	31	NW	NE.	NW.	calm	...	...	...	...	...	...
●	27	30 43	30 32	30 06	29 94	30·18	30 00	30 03	38	20	40	23	38	27	27	NE.	NE	N.	calm	...	·05	...	...	...	...
	28	30 43	30 13	29 92	29 90	30 10	29 98	29 92	43	40	50	28	45	41	31	SW.	S	SW.	S.	...	...	...	...	...	...
	29	30 13	30 00	29 70	29 60	29 90	29 84	29 12	48	32	50	41	50	41	36	SW	S.	SW.	S.	...	...	28	...	..	·91
	30	30 00	29 94	29 50	29 48	29 75	29 70	29 55	42	30	51	45	48	44	36	S.	SW.	S.	calm	...	...	...	...	...	...
	31	29 95	29 94	29 46	29 16	29 70	29 66	29 46	47	40	51	46	52	41	41	S.	SW.	S.	calm	30	05	02	0 325	010	..
Aver.		30 52	29 45	30 10	29 38	30 26	29 29	29 55	50	12	54	21	51	20	32 9					78	0 70	1 15	3 335	1 000	1 73

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

APRIL 1827.

L. *Description of a Horizontal Pumping Engine erected on the Mine of Moran in Mexico. By* PHILIP TAYLOR, *Esq.*

[With an Engraving.]

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE first steam-engine erected in the Real del Monte, was put in action on the 12th of last August, at the Mine of Moran. So novel a sight to the natives of Mexico, naturally attracted vast numbers of all ranks; and having heretofore seen no other means of raising water from their mines than such as were adapted to the comparatively feeble power of men or mules, they were of course astonished at the gigantic and untired efforts of one of these great servants to the arts.

As this engine differs in construction from any hitherto employed for pumping water, a short description of it may interest your readers.

I believe no doubt was ever entertained by those competent to form an opinion, that, if steam-engines could be transported to the mines of America, and fuel found to work them, such mines as admitted of their application would become far more productive. The difficulties anticipated were as to the conveyance of such ponderous machinery over so rugged a country, and as to the erection of it when arrived on the spot.

To obviate these difficulties as much as possible, I endeavoured to invent a powerful engine which at the same time should consist of such parts as would be easily conveyed, and so constructed that it could be erected and put to work without the usual labour and expense of building an engine-house, &c.

 In

In Plate I. is a section (fig. 1.) and a plan (fig. 2.), showing the principal parts of the engine now at work on the mine of Moran,—which was constructed by Messrs. Taylor and Martineau.

A A. The foundation on which the engine is fixed, being merely a level bed of masonry, with pieces of timber introduced to receive the bolts, &c. which hold down the engine.

B B. Two cylinders, each 10 feet in length and 18 inches interior diameter. These are fixed in a horizontal position and exactly parallel to each other, by means of the four cast-iron saddles C C C C, which embrace both cylinders, and are secured to the foundation.

Each of these cylinders has a metallic piston; one of which is shown (*a*) fig. 1. and it will be observed that both the pistons are fixed on the *middle* of the piston rods D D D D, which work through stuffing boxes at each end of the cylinders.

E E. Two strong cross heads, into which the four extremities of the piston rods are firmly fixed.

F F F F. Four friction wheels fitted on the ends of the cross heads. These wheels are grooved on their edges and traverse between parallel guide rods, which are kept in a state of tension by the screws at their extremities G G G G, their other ends being made fast to the saddles CC, which confine the cylinders.

H H. The connecting rods attached to the cross heads E E, by which the power may be applied to pumps placed either at one or both ends of the engine.

I I. The tappet rod fixed also to the cross heads E E, by the reciprocating motion of which the valves are opened and shut.

J J. The passages in the valve nozles to admit steam from the boiler.

K K. The passages through which the steam escapes after it has given motion to the piston.

The steam entering through the passages J J, is admitted by the action of the valves (*b*), to both cylinders at the same instant through the cross passages L L L L (fig. 2.) While in like manner the steam from the opposite ends of both cylinders passes off through the passages K K.

The pistons are 18 inches in diameter, and make a 9-feet stroke. The boilers attached to the engine are calculated to supply them with steam of a pressure equal to 50 pounds on the square inch with perfect safety.

The speed of the engine is regulated by a cataract, and the valves are so arranged as to allow of its being worked expansively or otherwise, as circumstances may render desireable.

able. These parts cannot be shown in a drawing on a small scale.

The arrangements which are more especially novel in this engine are, the mode of combining the effect of the cylinders, and the carrying the piston rods through both ends of the cylinders.

The horizontal position affords a facility of concentrating the power derived from even 4 or 6 cylinders upon one point, and the carrying the piston rods through both ends of the cylinders has the effect of preventing the weight of the piston from producing unequal friction, owing to the state of tension in which the rods are constantly kept.

It is obvious that with an engine thus constructed, the power may be divided and applied at each end; or it may be directed wholly to one end, by attaching at the opposite extremity a balance bob, or beam with a weight equal to half the power of the engine.

A common pumping engine with a beam requires that the engine-house should be built close to the mouth of the shaft in which the pumps are placed, which is often attended with much inconvenience. The engine above described may be merely covered by a shed, and this placed at any convenient distance from the shaft. As mining engines are often removed from one situation to another, the greater the facility of fixing them, and the less masonry required, the more will time and expense be saved.

The engine which I have described, with three others built in Cornwall under the superintendence of Mr. Woolf, and a complete out-fit of founders', engineers', and millwrights' tools, implements, &c., also saw-mills and stamping-mills, were shipped on board the Melpomene, at Falmouth, on the 30th of March 1825, and arrived off the coast of Mexico the 27th of May following.

The Castle of St. Juan de Ulloa, which commands the harbour of Vera Cruz, being at that time in the possession of the Spaniards, the cargo was obliged to be landed on the beach at Mocambo, a league to the southward, which could not be accomplished until the 10th of June, when the setting in of the rains, and of the unhealthy season on the coast, occasioned great suffering and the death of some of the transport party. These circumstances prevented the machinery being carried further than Santa Fé, which is about four miles from the coast.

Here Lieut. Colquhoun of the Royal Artillery, who had taken the charge of this most arduous enterprise, remained to recruit the health and strength of the party under his command,

mand, as well as to procure mules and make arrangements for moving up the country on the return of the dry season. In the January following the great bulk of the machinery was forwarded by different convoys to a depôt on the table-land near Jalapa; and on March 31st, a train of 52 waggons, carrying the engine above described, with the various other articles, proceeded to the Real del Monte, and reached the Mine of Moran, on the 1st of May 1826. It has been already stated that this engine was in action on the 12th of August, an instance of dispatch which does great credit to Mr. Blackaller who had the charge of erecting it, under the orders of Captain Vetch, the first commissioner of the Real del Monte company.

The following particulars are from the letters of a gentleman who was present when the engine went to work.

"The engine went off in great style with 20 pounds steam, and very soon brought the water up to the launders to the surprise of the native spectators of all classes, who were greatly astonished at this visible proof of her power. In 40 minutes she lowered the water in the shaft 10 inches. Before connecting with the bobs, we had tried her friction as the boilers heated; she began to move with $2\frac{1}{2}$ pounds steam."

"From the 12th of August to the 7th of September the engine continued to work regularly, as far as the repairs of the shaft would permit; it being necessary to remove decayed timber, and replace it with new, clear obstructions, and drop the pumps from time to time as the water lowered. The average time of the engine working amounted to about six hours per day—the steam in the boiler being at 25 pounds pressure—worked expansively—the steam valve closing at about half stroke. At the above date the water was drawn out to the depth of 18 varas (the vara being nearly a yard)."

A later account mentions that on the 24th of September the water was lowered to 45 varas, which is one half the depth of the mine.

There is every reason to think from these statements that when this mine is once drained, it will be easily kept clear of water.

The fuel employed under the steam boilers is small oak with a little pine, which is so abundant that its cost will not exceed that for coals in Cornwall.

I am, gentlemen, yours very truly,

Jan. 22, 1827. PHILIP TAYLOR.

P. S. The following is an extract from a letter addressed to Messrs.

Messrs. Taylor and Martineau, by Mr. J. Blackaller, and dated Real del Monte, Oct. 18, 1826.

"In the early part of May last I had the pleasure of having the erection of the first of your engines at the mine of Moran placed under my direction and superintendance; and on the 12th of August started the same, to the no small surprise and satisfaction of the numerous visitors who had assembled on the occasion. The engine has continued to work beyond our most sanguine expectations, not a thing having failed or required alteration.

"Our foundry has been at work a short time, and we have turned out some decent castings, both in brass and iron."

On the 31st of October, Captain Vetch also writes: "I am happy to state that Moran Mine may now be considered as dry; that is, the water has been sunk to the bottom of the shaft; but it will be necessary by means of flat rods to drain some of the *pozos* (pits or winzes) in the lowest level, to get at the rich ores."

LI. *Analysis of a Sulphuretted Water from the Northern Part of the Yorkshire Coal-field.* By E. S. George, *F.L.S. Hon. Mem. Y.P.S.**

THIS mineral water is very extensively employed in the fulling of woollen cloths,—a process to which, from the absence of earthy salts, it is peculiarly adapted. It formerly issued in a considerable spring at the village of Holbeck near Leeds, and was used medicinally: it appears in most cases to rise from a thick bed of shale lying below the flagstone, and so large is the supply that it has been procured in every situation in which borings to a sufficient depth have been made. There are in Leeds near fifty borings, and about 200,000 gallons of the water are pumped up daily. The depth at which the water is procured, from 70 to 200 yards, according to the situation of the well as regards the inclination of the strata. The amount of both gaseous and saline contents varies with the occurrence of higher springs, affected by heavy rains or by sudden elevations of the river Aire.

The water analysed was from Johnson's Well in Campfield, Leeds; the depth of the boring 90 yards: upon the surface is a bed of gravel about four yards thick, communicating with the river Aire, from which the well is about 200 yards distant: the water in the gravel is prevented from mixing with that in

* Read to the Yorksh. Phil. Soc. Jan. 2, 1827; and communicated by the Author.

the

the boring by a casing of iron pipes descending into a solid stratum of shale.

Analysis.

Specific gravity 1·00155.

The water when taken from the spring appears clear and sparkling, has a considerable sulphuretted odour, slightly blues reddened litmus; reddens turmeric; with solution of soap gives a very slight curdy precipitate. Lime-water occasions a precipitate both before and after ebullition. Oxalate of ammonia no precipitate; in the concentrated water a slight cloudiness. Nitrate of barytes, a precipitate; the addition of a few drops of nitric acid removed nearly the whole with effervescence. Nitrate of silver, a precipitate with an immediate slight discoloration. Ferrocyanate of potash, no precipitate, nor any change of colour. Acetate of lead, a copious precipitate with a brown tinge, before boiling,—a colourless precipitate after ebullition.

Nitro-muriate of platinum does not occasion any precipitate in the concentrated water after boiling. The concentrated water possesses a strong alkaline taste, and deeply reddens turmeric paper: not the slightest cloudiness was perceptible after boiling the water twenty minutes with the addition of carbonate of soda. Phosphate of soda gave no indication of magnesia.

The action of tests shows that sulphuretted hydrogen exists in a gaseous state, that the carbonate, muriate, and sulphate of an alkali are present, and that the alkali is soda; that the water does not contain any metallic salt, any muriate of lime, or any salt of magnesia.

Gaseous contents.

Having ascertained the gases contained in the water to be sulphuretted hydrogen, carburetted hydrogen, oxygen and azote;—a wine gallon of the water was boiled in a proper apparatus, and gave out 12 cubic inches of gas.—The experiment was repeated several times with the same result.

1. To ascertain the amount of sulphuretted hydrogen, exposed a cubic inch of the gas, contained in a graduated glass tube, to contact, by very gentle agitation, with carbonate of lead; (obtained by precipitating [the carbonate of lead] from a solution of acetate of lead by carbonate of ammonia as directed by Dr. Henry, in his excellent paper "On the analysis of coal gas") 0·22 of a cubic inch was absorbed. Having thus separated the sulphuretted hydrogen, agitated the residue in a solution of caustic potash, a further diminution of 0·18 of a cubic inch of carbonic acid gas took place.

2. 0·33

2. 0·33 of a cubic inch of the gas, (after the separation of the sulphuretted hydrogen and carbonic acid gases,) was mixed with 0·60 of a cubic inch of nitrous gas: an absorption of 0·10 took place, indicating 0·025 of oxygen gas; and in 1 cubic inch of the gas from the water, 0·045 of oxygen gas.

3. On firing 0·5 of a cubic inch of the unabsorbable gases with 0·575 of oxygen, the carbonic acid produced was 0·20, and the oxygen that had disappeared 0·397; approximating so closely to the quantity of carbonic acid gas produced, and of oxygen gas consumed during the detonation of the carburetted hydrogen gas from stagnant pools, that the composition of the gases may be considered the same; and since that gas produces its own bulk of carbonic acid, the carburetted hydrogen contained in 0·50 of the gas will be 0·20, and in 1 cubic inch of the gas from the water, will be 0·24.

4. After the separation of the excess of oxygen by nitrous gas the residual azote was 0·2625. The gases contained in the water are:

	In one cubic inch of the gas. Cubic Inches.	In a gallon of the water. Cubic Inches.
Carburetted hydrogen	0·24	2·88
Sulphuretted do.	0·22	2·64
Carbonic acid	0·18	2·16
Oxygen	0·045	0·54
Azote	0·315	3·78
	1·000	12·00

Saline contents.

1. Evaporated one wine-gallon of the water gradually to dryness; it did not become in the least turbid: the concentrated liquid tasted strongly alkaline, the solid residue weighed 38·5 grains.

2. Dissolved the 38·5 grains in 8 ounces of distilled water, and boiled a few minutes; a small portion of flocculent matter floated in the solution; separated by subsidence and dried, it weighed 0·3 grains.

3. Into the clear solutions dropped nitrate of barytes as long as any precipitate fell down; collected by subsidence and dried after repeated washings, it weighed 60 grains: this precipitate contained the carbonic and sulphuric acids existing in the water.

4. Upon the precipitate (No. 3.) poured dilute nitric acid: nearly the whole was dissolved with effervescence; the insoluble part when dried weighed 9 grains, and was sulphate of barytes.

5. Into

5. Into the solution after the separation of the precipitate (No. 3.) dropped a solution of nitrate of silver: a precipitate of chloride of silver fell down, weighing after fusion 7·9 grs.

6. Upon the 3 grains insoluble in water (No. 2.) poured dilute nitric acid: the whole was dissolved with effervescence, and was again entirely precipitated by oxalate of ammonia.

The contents of the water are:

Carbonic acid in 51 grains of carbonate of barytes and 0·3 gr. of carbonate of lime . . .	11·40
Sulphuric acid in 9 grs. of sulphate of barytes .	3·05
Muriatic acid indicated by 7·9 grs. of chloride of silver	1·97
Soda	20·48
Lime	·17

Existing in the water as

Carbonate of soda	27·40
Sulphate of soda	5·00
Muriate of soda	4·25
Sulphate of lime	0·41
Loss	1·44
	38·50

The loss appears to be partly occasioned by the different hygrometric states of the substances; the salts resulting from evaporation being highly deliquescent, and requiring great care during their evaporation, to prevent the decomposition of the carbonate of soda: while the precipitates of carbonate and sulphate of barytes and chloride of silver, retaining moisture with much less tenacity, were dried at a higher temperature.

It is probable that the soda exists in the water as a bicarbonate, the excess of carbonic acid being given out in the state of gas, during the evaporation required to perform the analysis. The water examined presents two striking peculiarities, the large quantity of carburetted hydrogen, (this gas although supposed to occur in mineral waters, was only very lately proved to exist, by the experiments of Mr. West, on the water of the Crown Spa at Harrogate,) and in the saline contents the large amount of carbonate of soda. I shall not offer any conjectures on its source; but only remark, that I have detected its existence in many waters of the Yorkshire Coal-district.

St. Peter's Hill, Leeds,
Jan. 1, 1827.

LII. *Ap-*

LII. *Application of the Variations of Temperature in Air that changes its Volume to account for the Velocity of Sound.* By J. Ivory, *Esq. M.A. F.R.S.**

THE formula for the velocity of sound investigated by Newton, having finally overcome all objections, it still remained to account for the remarkable discrepancy between the theory and observation. The difference, amounting to a sixth of the whole quantity, could hardly be thrown entirely upon incidental errors of the experiments. The author of the *Principia* led the way in the conjectures that were advanced for reconciling the calculated velocity of sound with the true velocity; but as all these attempts have shared the usual fate of hypotheses, and have lost all interest by the discovery of the real cause, it would be superfluous to mention them here. But it will be proper to observe, that the difficulty was occasioned by no inaccuracy or neglect of Newton. It arose from an inexact estimate of the air's elasticity, which he was unavoidably led to make from the state of natural science in his time, and which the progress of knowledge has enabled the philosophers of the present day to correct. When the exact elasticity is substituted for the inaccurate quantity, the discrepancy between theory and experiment disappears, without any change being required in the demonstration. At the time of the publication of the *Principia*, and long after that time, what could possibly have led any one to surmise, that nearly half as much heat enters into air when it dilates, and comes out of it when it contracts, as must be applied from some extraneous source, in order to produce the same change of volume?

The fact, that air absorbs heat when it expands, and evolves heat when it contracts, having been established by many experiments; and very notable effects being observed in some cases of great and sudden condensation; Laplace, between 20 and 30 years ago, first suggested that this property of air was the cause of the perplexing difference between the velocity of sound as determined by theory and observation. In the aërial undulations by which sound is conveyed to the ear, every small portion of air is first condensed and then dilated; and we may compare the elasticities on the two suppositions, that the temperature of the agitated air remains the same as in the quiescent state of equilibrium, and that it varies with the changes of volume. The external compressive force being always the same, it is manifest that, whenever the bulk of

* Communicated by the Author.

the

the small parcel of air is less than in the quiescent state, the elasticity will be greater on the second supposition than on the first, on account of the extrication of heat; but, whenever the bulk is greater than in the quiescent state, the elasticity will be less on the second supposition than on the first, on account of the cold produced by the absorption of heat. Now the accelerating forces of the aërial particles are the differences between their actual elasticity and the elasticity of the quiescent medium; and as these forces are always greater on the second supposition than on the first, the velocity of the undulation must be swifter in that case than in this. The formula of Newton, being deduced from the law of Boyle and Mariotte, is consonant to the first supposition; and there is undoubtedly in the second supposition a tendency to diminish the difference between theory and experiment, by increasing the estimate of the velocity of sound. One circumstance however, it may be alleged, must in some degree modify the effect of the variations of heat in the agitated air; namely, the rapidity with which the small increments and decrements of free heat would be equalized to the temperature of the surrounding medium. But the whole time of an aërial vibration is extremely short; and we may safely consider every change of volume that takes place during its progress, and every variation of free heat, as enduring only for an indivisible instant of time. Every parcel of air as it is successively agitated retains the whole of its absolute heat; and the rapid evolution and absorption of free heat have no other effect than to increase the elasticity.

The principle suggested by Laplace, having a real existence in fact, and being adequate at least in a certain degree to reconcile the theory with experiment, it became important to ascertain the exact increase of velocity deducible from it. But here a difficulty occurred. It was known that heat was extricated from air when it is condensed, but there was no measure of the effect. It even seemed very difficult, if not impossible, to arrive at any tolerably precise estimation by direct experiment. MM. Biot and Poisson therefore reversed the question, and inquired in what degree the elasticity computed by the law of Boyle and Mariotte must be increased; or, which is the same thing, in what proportion the free heat must vary with respect to the volume; in order to bring out the true velocity of sound. By this means we might at least be able to judge whether the assigned cause would alone account for the observed deficiency. And, admitting that the effect fell within the limits of probability, there can be no doubt that the just rules of philosophizing would be nowise infringed

infringed by adopting the explanation deduced, by this inverted procedure, from the phænomenon itself. In 1816 Laplace published the following theorem, without the demonstration:—*The velocity of sound is equal to the velocity according to Newton's formula, multiplied by the square root of the proportion of the specific heat of air under a constant pressure, to the specific heat under a constant volume.* The investigation was first given in the *Conn. des Tems* 1825, and afterwards in the xiith book of the *Mécanique Céleste.* This theorem left nothing more to be done than to find a certain ratio in numbers; and this was accomplished by the ingenious experiment of MM. Clement and Desormes, from which we have deduced the proportion of the latent, to the free, heat, when air varies under a constant pressure. MM. Gay-Lussac and Welter improved a little the original procedure of the inventors, and repeated the experiment in a great variety of circumstances; by which means they not only determined the number sought more exactly, but they likewise showed that it was constant, or nearly so, in considerable diversity of temperature and pressure. The result of this long investigation, protracted for so many years, was a complete solution of the difficulty, and a satisfactory reconcilement of the theoretical, with the experimental, estimate of the velocity of sound.

The numerical value of the proportion indicated in Laplace's theorem is immediately deducible from what has been shown respecting the manner in which heat combines with elastic fluids. When air varies under a constant pressure, the absolute heat requisite to produce the rise of temperature τ, is $\tau + i$, i being the latent heat. But τ is the heat that causes an equal rise of temperature when the volume is constant. It is manifest therefore that the proportion of the two specific heats in the theorem, is $\tau + i$ to τ, or $1 + \frac{i}{\tau}$ to 1, that is, $1 + \frac{\alpha}{\beta}$ to 1: and $\sqrt{1 + \frac{\alpha}{\beta}}$ is the factor by which the Newtonian velocity of sound must be multiplied, in order to obtain the true velocity.

But the whole difficulty respecting the velocity of sound is overcome, when it has been found how much heat is extricated from air condensed in a given degree. This is the leading principle on which the investigation must turn, by whatever process the result is brought out. In Newton's formula the pressure and density are supposed to follow the law of Boyle and Mariotte; and the computation will be best rectified by searching out the true relation of the same quantities, and substituting it in the place of that inaccurately employed. It

remains, then, to investigate the relation between the elasticity and density of a mass of air that varies its temperature as it dilates and contracts, without losing or receiving any heat from the surrounding medium.

Put p', ϱ', θ, for the pressure, density, and temperature of a given mass of air; and suppose that these quantities are simultaneously changed into p, ϱ, $\theta + \tau$; then, we shall have,

$$\frac{p}{p'} = \frac{\varrho}{\varrho'} \cdot \frac{1 + \alpha\theta + \alpha\tau}{1 + \alpha\theta}.$$

Again, p' remaining the same, put D for the density at the beginning of the thermometrical scale; and let i' be the latent heat requisite to change D into ϱ': then

$$\frac{\varrho'}{D} = \frac{1}{1 + \alpha\theta},$$

$$\frac{\varrho'}{D} = \frac{1}{1 + \beta i'}.$$

Further let $i' + i$ be the latent heat accompanying the change of D into ϱ; and,

$$\frac{\varrho}{D} = \frac{1}{1 + \beta i' + \beta i} = \frac{1}{1 + \alpha\theta + \beta i}.$$

Hence,
$$\frac{\varrho}{\varrho'} = \frac{1 + \alpha\theta}{1 + \alpha\theta + \beta i}.$$

From the values that have been found, we now get,

$$\left.\begin{aligned} \frac{p}{p'} &= \frac{1 + \alpha\theta + \alpha\tau}{1 + \alpha\theta + \beta i} \\ \frac{\varrho}{\varrho'} &= \frac{1 + \alpha\theta}{1 + \alpha\theta + \beta i} \end{aligned}\right\} \quad \text{(C)}$$

These formulæ express the elasticity and density of the air by means of the initial quantities p', ϱ', θ, and the variations of temperature and latent heat represented by τ and i. It must be observed, however, that the mass of air is supposed to vary in an unlimited supply of heat; so that the small increments and decrements of free heat arising from the changes of volume produce no effect on the thermometer, being continually equalized to the temperature of the surrounding bodies. In this case the quantities τ and i are independent on one another; the first being the temperature as shown by the thermometer, and the second the latent heat connected with the change of bulk. But if the supply of heat were limited, it would be requisite to take into account the free heat evolved or absorbed by the contraction and dilatation of the air. For this purpose we must write $\tau - i$ for τ in the first of the formulæ (C); supposing that τ is all the heat derived from extraneous sources, and $+ i$ the variation of the latent heat. In

a parcel

a parcel of air agitated in an aërial undulation, there is no extraneous heat, and $\tau = 0$: the foregoing equation, therefore, will become,

$$\frac{p}{p'} = \frac{1 + \alpha\theta - \alpha i}{1 + \alpha\theta + \beta i},$$

$$\frac{\varrho}{\varrho'} = \frac{1 + \alpha\theta}{1 + \alpha\theta + \beta i}.$$

And, by exterminating i,

$$\frac{p}{p'} = \frac{\varrho}{\varrho'}\left(1 + \frac{\alpha}{\beta}\right) - \frac{\alpha}{\beta}. \qquad \text{(D)}$$

This equation expresses the relation between the elasticity and density in the circumstances supposed, and it is that which must be employed in the investigation of the velocity of sound in place of the equation $\frac{p}{p'} = \frac{\varrho}{\varrho'}$, resulting from the law of Boyle and Mariotte, and forming the basis of Newton's formula.

In the Philosophical Magazine for June 1825, p. 12, the following equation is obtained in considering the motion of a line of air, viz.

$$\frac{\varrho}{\varrho'} = 1 - \frac{dz}{dx}:$$

Substitute, now, this value in equat. (D); thus

$$\frac{p}{p'} = 1 - \left(1 + \frac{\alpha}{\beta}\right)\frac{dz}{dx}:$$

and if we put $k = 1 + \frac{\alpha}{\beta}$, and go through the rest of the calculation as at the place cited, we shall obtain,

$$\frac{ddz}{d\tau^2} = k \times \frac{p'}{\varrho'} \times \frac{ddz}{dx^2}.$$

The true velocity of sound is therefore $\sqrt{k \times \frac{p'}{\varrho'}}$: but the Newtonian velocity, deduced from the law of Boyle and Mariotte, is $\sqrt{\frac{p'}{\varrho'}}$: and these two formulæ contain the demonstration of Laplace's theorem.

The theory we have attempted to give of the combination of heat with elastic fluids is founded on acknowledged facts. It is general, extending as far as experience has enabled us to reduce the effects of heat to precise rules. It follows from it that the quantity k, on which the velocity of sound depends, has the same value for air and all the gases; and likewise that it remains constant in every diversity of pressure and density: all which consequences are known to be consonant to observation.

The

The equation (D) does not coincide with what is elsewhere given for expressing the relation of the same quantities. It is different from the equation published by M. Poisson in the *Conn. des Tems* 1826, p. 264. In order to clear away all clouds of obscurity from a matter of considerable importance, I shall now examine particularly, what it is that occasions the difference. For this purpose I shall set out from M. Poisson's equation (6), p. 263, viz.

$$\omega = (k - 1)(1 + \alpha\theta)$$

Here ω is the variation of latent heat corresponding to the small condensation γ; and, in our notation, $\omega = di$, $\gamma = \frac{d\varrho}{\varrho}$, $k - 1 = \frac{\alpha}{\beta}$: the equation may now be put in this form, viz.

$$\frac{d\varrho}{\varrho} = \frac{\beta\, di}{1 + \alpha\theta},$$

which is nowise different from what M. Poisson obtains in p. 264, except that he writes $d\theta = \omega$, instead of $di = \omega$. Differentiate the second of the formulæ (C), changing the sign of i in order to agree with M. Poisson's supposition, that the density increases; then,

$$\frac{d\varrho}{\varrho} = \frac{\beta\, di}{1 + \alpha\theta - \beta i}.$$

Now this equation is identical with M. Poisson's only at one point, namely, when $i = 0$. The latter is therefore true only in a particular state of the variables, and is inexact in all other circumstances. When the density and latent heat of a mass of air vary together, M. Poisson's equation expresses the true relation of the differentials only initially; and it ceases to be exact when the variable quantities have changed their original magnitudes. The integral formulæ deduced from such a process cannot be accurate results, although they may be approximations. The truth of what has been observed must be so evident to any one that will consider with attention the manner in which the author obtains the equation in question, that it would be a waste of words to attempt any further explanation.

The investigation I have given in pp. 7 and 8 of the Phil. Mag. for June 1825, is liable to the same objection that has just been urged against M. Poisson. The relation of the differentials is obtained only in a particular state of the variables. The experiment of MM. Clement and Desormes, although it enables us to ascertain the value of $\frac{\alpha}{\beta}$, is, nevertheless, in-

sufficient

sufficient for finding, generally, the relation between the density and latent heat, when these quantities vary together.

It must not, however, be imagined that the damage arising from the inadvertency that has been noticed, is ruinously great. The formulæ obtained are true to quantities of the second order with respect to α and β. They are sufficiently exact for investigating the velocity of sound; and they can hardly lead to any error of moment in any practical inquiry. But it is always best to square our speculations according to experience and the laws actually followed in nature; and, in a case like the present, when it may be supposed that we have returned into the right path after having deviated a little from it, it is instructive to look back and examine what led us astray.

In further illustration of what has been said, it may not be improper to add a few words concerning the equations in the xiith book of the *Mécanique Céleste*, pp. 123, 127. For this purpose I seek the values of τ and i from the foregoing equations (C); then, by taking the sum, we get,

$$\tau + i = \mathrm{V} = \left(\frac{p}{p'} \cdot \frac{\varrho'}{\varrho} - 1\right)\frac{1 + \alpha\theta}{\alpha} + \left(\frac{\varrho'}{\varrho} - 1\right)\frac{1 + \alpha\theta}{\beta}.$$

Put $k = 1 + \frac{\alpha}{\beta}$ as before, and differentiate: then

$$\frac{d\mathrm{V}}{d\varrho}\varrho + k\frac{d\mathrm{V}}{dp}p = \frac{1 + \alpha\theta}{\beta} \cdot \frac{\varrho'}{\varrho} \cdot \frac{p - p'}{p'}.$$

We have initially, $p = p'$, $\varrho = \varrho'$; and if we suppose that the mass of air undergoes only a small variation from the initial state, we shall have,

$$\frac{d\mathrm{V}}{d\varrho}\varrho + k\frac{d\mathrm{V}}{dp}p = 0$$

$$k = \frac{-\frac{d\mathrm{V}}{d\varrho}\varrho}{\frac{d\mathrm{V}}{dp}p}.$$

These equations are true only at one point, and in a particular state of the variables, as has been mentioned. They can have nothing to do with integration, which supposes that the differential equations are exact for all values of the flowing quantities within the limits of their variation. They merely express that the two specific heats, under a constant pressure and under a constant volume, have to one another the same invariable proportion, whatever be the condition of the mass of air.

March 5, 1827. J. Ivory.

LIII. *Theory*

LIII. *Theory of the Spirit-Level.* *By* J. Nixon, *Esq.**

Definitions†.—1. THAT part of the straight (or perpendicular) line in which the plummet hangs, and bodies fall to the ground, which lies above any given point (on the earth's surface) through which it passes, is termed the vertical (line) of that point, and terminates upwards at another point called the vertex or zenith. 2. A straight line, or plane passing through the given point at right angles to its vertical, is termed a horizontal line or plane. 3. As no one vertical is parallel to another, the horizontal lines or planes of points situated in the same vertical, although parallel to each other, are nevertheless inclined to those of points lying in any other vertical. 4. Planes which pass through the given point and that of its zenith (in the direction of the vertical line in which they are situated), cut its horizontal plane at right angles, and are called vertical planes. 5. As the angle formed at the given point by the intersection of its vertical and a straight line from any other point will lie in a vertical plane, it is termed a vertical angle, and is equal to the zenith distance of that point or line. 6. The vertical angle formed at the given point by the intersection of one of its horizontal lines and a straight line produced from any other point, is equal to the horizontal inclination of that line. 7. This vertical angle is also termed the elevation or depression of the same line or point, accordingly as it is situated above or below the horizontal plane (or horizon) of the given point. 8. The zenith distance of a horizontal line or plane, and the elevation of the zenith of any given point being equal to each other and to a right angle, it follows that the angle of elevation of any other point or line is equal to 90° *minus* its zenith distance, and that of depression to the zenith distance *minus* 90°.

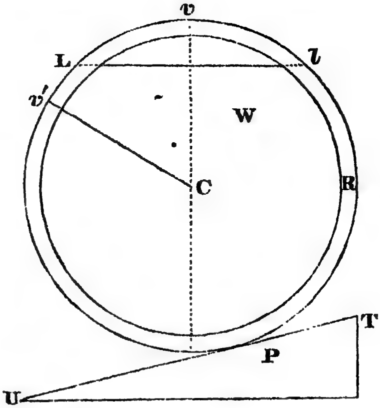
9. Fluids gravitate in straight lines in the direction of gravity. 10. When at rest, and subjected to the sole action of gravity, their order of superposition, with curved surfaces of contact, is inversely as their specific gravities. 11. The horizontal lines or plane of any given point situated on the surface of a fluid, having other fluids superincumbent or not, are tangents to that surface. 12. When the surface of the fluid is of inconsiderable extent, it is sensibly a horizontal plane, perpendicular to the vertical and parallel to the horizontal lines and plane of any other point above or below it.

* Communicated by the Author.

† The exact figure of the earth's surface is supposed to be unknown.

If

If we place in a vertical position the sides of a (glass) vessel W, formed of two equal parallel circular planes, held together by a rim R, perpendicular to the planes, the surface of contact of the incompressible fluid (or liquid), with the superincumbent elastic fluid, together filling the vessel, will be sensibly a horizontal plane. A vertical plane passing in the direction of the centres of the circles, (through their vertices and that of the rim,) will divide at right angles a



straight line L*l*, drawn on this horizontal surface parallel to the circles, into two equal parts. The vertex or zenith *v* of the circumference of either circle or the rim will therefore be the point of bisection of such part of the arc of either as is situated *above* this horizontal surface.

Having marked this zenith-point, if the vessel be made to describe in a vertical plane, any part of a revolution about the horizontal line or axis C, passing through the centres of the circles, the mark moving along with the vessel, will pass over an equal arc of revolution. The zenith-distance of a straight line drawn from the mark, which we will now call *v'*, to C, will therefore be equal to that arc or to the angle formed by the intersection at C of this straight line, and a vertical line passing through the *new* zenith-point of the rim, &c. found by bisecting, as before, the arch of the rim, &c. *now* extended over the horizontal surface L *l*.

When the interior of the rim is perfectly circular, the arc passed over (or zenith-distance of *v'*) may be measured at once on its graduated parallel exterior. But should the figure of the rim be that of *any* other curve, the length of L *l* will vary in different parts of the curve;—the zenith-points will seldom be vertical to the point of bisection of L *l*, or be situated at the middle point of the arch extended over it. The points *v* and *v'* must now be found exclusively by drawing straight lines through the centre of revolution (or axis) of the vessel perpendicular to L *l*; their angular opening or zenith-distance of *v'* being measured on a graduated circle described

 on

on either vertical side of the vessel concentric to the axis of rotation.

Vertical lines passing through points situated near each other, differ so slightly in parallelism, that the vessel might have been moved forty horizontal feet in the interval of marking the two zenith-points, without introducing an error in the zenith-distance of v' equal to half a second. If the expansion of the liquid should exceed that of the glass of the vessel, the increase of temperature which augments its volume will elevate its horizontal surface, and cause a sensible diminution in its area and the length of the line L l. Decrease of temperature will therefore augment the horizontal surface, and elongate L l. In either case the zenith-point will be invariably at the point of bisection of the arch situated above the surface of the liquid, without regard to its extent.

When the temperature of every part of the vessel is not the same (which may be the result of handling it, or breathing on it), the circular figure is destroyed, and the length as well as probably the figure of the surface of the liquid undergo alterations. The graduations of the distorted rim are therefore rendered unserviceable from their inequality; and should the partial temperature affect the vessel where in contact with the surface of the liquid; in such case the true zenith-point cannot be found otherwise than by drawing a line through C perpendicular to L l. Generally the surface of the liquid will appear to advance towards that part of the rim bulged out by the partial increase of temperature.

In the construction of a spirit-level the upper interior surface of a straight (hollow) cylinder of glass is ground in the direction of its axis to a perfectly circular arch. Either end being permanently closed, the cylinder is *nearly* filled with spirits of wine or ether, and the other end hermetically sealed. Hence it is evident that any section of our circular vessel perpendicular to its sides, when nearly filled with the proper liquid, and securely closed up, would be equivalent to a similar spirit-level. In this instrument the atmospheric air incumbent on the ether, &c. or rather their surface of contact, is termed the air-bubble, or simply the bubble; and is represented in our circular vessel by the horizontal surface of the liquid on which is drawn the straight line L l.

Having learned from our experiments with the circular vessel, that the length (or figure) of the bubble L l would not alter in a constant temperature, we may restrict ourselves, in lieu of finding the zenith-points v and v', to the marking of either end of the bubble, as L or l, *before*, and the *same* end

subsequent

subsequent to any degree of revolution of the vessel. The zenith-distance of v', equal to its change of inclination, may now be ascertained at once, by observing on the graduated rim, &c. the angular distance of the two marks. To guard against any change of temperature in the interval of the operation, it will be advisable to mark the rim at both ends of the bubble (L and l) and to consider the half-sum of the degrees, &c. on the rim corresponding to each mark as the zenith-point of v (or v'). The difference of these two half-sums, granting the change of temperature to have been uniform, will give the zenith-distance of v'. When the graduations are sufficiently large to admit of being read off without vernier, &c. we may dispense with the marks by noting the degrees, &c. exactly over the ends of the bubble.

In order to graduate in a similar manner the upper or convex surface of a spirit-level, we must ascertain in the first place the linear space passed over by its bubble, corresponding to a certain angular change of inclination, as one minute, one second, &c. This may be effected after various methods *; as by fixing the level t a long straight bar of a known length, and having elevated either end a quantity equal to the given angle, to note the inches, &c. passed over by the bubble; generally termed its displacement. The tube or the ivory scale laterally attached to it, may now be divided into equal spaces †, each equivalent to a change of inclination of one second, &c.; and so numbered as to give, without risk of mistake, the middle point of the bubble, regardless of its varying length, and consequently minute differences of vertical angles ‡.

* The French verify the scale of the great level of their repeating-circle, by measuring on its graduated vertical circle a sufficient multiple of the minute angle subtended by two well-defined fixed objects situated in the same vertical plane, and comparing the result with the corresponding measurement by the divided scale of the level.

In lieu of the repeating-circle we might have recourse to the micrometer of a telescope.

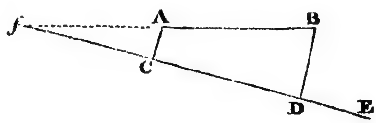
† When the bubble does not pass over equal spaces for equal angles of inclination throughout the length of the tube, it proves that the arch is not perfectly circular.

‡ In general the middle division is considered as zero, whence the numeration, alike for the divisions on each side of it, is carried on progressively to each end of the scale.—The divisions on the one side of zero being considered as positive, and those on the other side as negative, the distance of each end of the bubble from zero, with the proper signs prefixed, are registered; and half their sum, when the signs are alike, or half their difference (with the sign of the greater quantity), when the signs are unlike, is considered the middle point of the bubble, or vertex of the level. Hence the difference or sum, as their signs are like or unlike, of two similar middle points, of which one was noted before, and the other after an alteration of inclination of the level, indicate its angular value.

The

The radius of curvature of a spirit-level is found by multiplying the linear displacement of the bubble, answering to a change of inclination of one second, by 200,000; and vertical angles are measured on the divided scale of a spirit-level as correctly as by a plumb-line of the length of the radius of curvature of its tube*. (Had the cylindrical tube of the level been without curvature, and closed at the ends with (circular) planes perpendicular to the axis of the cylinder, the bubble, if we may designate the horizontal surface of the liquid as such, would extend from one end of the tube to the other, and small angles of inclination, measured on a divided vertical line or scale passing through the centre of each end, would possess a degree of accuracy equal only to similar observations made with a plumb-line of the length of the cylinder.)

The circular vessel W, is represented in the figure as fixed to a pedestal (P) of a triangular shape; and we might imagine, that increased temperature, inasmuch as it would elevate T without affecting the height of U, would produce a greater inclination of the plane TU, and consequently throw vC out of perpendicular. But as the sides of the triangle will elongate in one and the same ratio, the angles they subtend, and therefore the horizontal inclination of TU, must be constant. Hence the *exterior* sides of the tube, instead of being parallel, might be inclined to the cylindrical interior without disturbing, during variations of uniform temperature, the position of the bubble. Supposing even the *interior* of the tube to be conical, it does not follow, as might at first be conjectured, that change of temperature would affect the inclination of the level.



Let ABCD represent the vertical section of a conical tube resting on the plane E, which forms an angle with the horizon equal to the inclination of the sides of the cone (or BfD), so that the upper side of the cone AB will be horizontal. Then if the four sides of the section augment (from expansion) in the same ratio, the

* The linear displacement per second of the bubble of a spirit-level seldom exceeds the one-twentieth part of an inch; but occasionally, especially on the continent, they have been constructed with a much greater radius of curvature. In a level by Reichenbach it amounted to 200 miles!

angles

angles at A, B, C, D will be constant, and the upper side AB preserve its parallelism to the horizon*.

When the temperature of the level is not uniform, the bubble (as in the parallel case of the circular vessel) will be displaced and move towards the warmer end of the tube. Its ground arch, as well as the divisions of the scale being distorted, the half-sum of the divisions at each end of the bubble cannot correspond with the true vertex of the level.

The tube of the level is generally mounted in a thin case of brass, a metal which expands in a greater ratio than glass. When the bottom of the case and that side of the tube in contact with it are not strictly parallel, it may occur in great variations of temperature that the tube will rest on some different part of the case, and cause a sensible variation in its inclination to the horizon. The difference of expansion may also affect the radius of curvature, or alter the perfectly circular figure of the arch of the tube.

In our circular vessel, as the graduated rim is perpendicular to its horizontal axis, either end of the bubble (or horizontal line L l) must describe, as the vessel revolves, arcs of a circle lying in a vertical plane: and were a hollow (glass) sphere, nearly filled with any liquid, made to complete a revolution about a horizontal line (or axis) passing through its centre, then would the centre of the surface of the liquid (or bubble) have described a great circle, also lying in a vertical plane perpendicular to the axis, and on which, when graduated, zenith-distances, &c. might be measured, precisely the same as on the rim of the circular vessel. And if we divide the interior of the sphere into two *unequal* parts by means of a circular plane (inferior in diameter to the sphere) also placed at right angles to the axis of rotation, and nearly fill the two divisions with any liquid, then will a vertical plane passing in the direction of the axis of the sphere, divide at any period of its revolution the semicircular bubble of the lesser division, and the circular one of the larger division (moving parallel to each other) into two equal parts. Hence differences of inclination, &c. measured on the great circle, or on the similarly graduated rim of the parallel circular plane would always be equal.

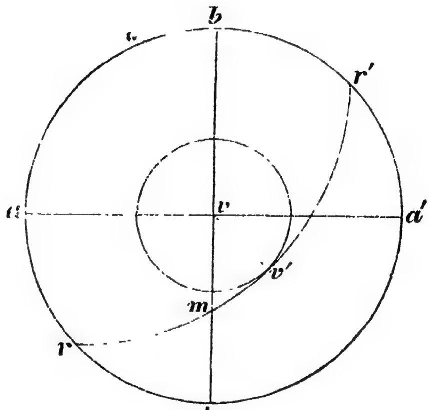
Let a graduated hollow glass ring, nearly filled with any liquid, be made to closely incircle the sphere (the partition being withdrawn) in the direction of any one of its great circles except the one perpendicular to its axis. When the point of intersection of these two circles is made to coincide with the

* It is nevertheless *certain* that change of temperature alters the vertex, or reversing point of most spirit-levels; which the artists attribute to the tubes not being perfectly cylindrical.

vertex

vertex of the sphere, then will the centres of the bubble of the ring and that of the sphere also coincide, or be in the same vertical; yet it will be found after any partial revolution of the sphere, that although the bubble of the ring will always come to rest at the most elevated point of the ring, or that part of it the nearest to the vertex of the sphere (or centre of its bubble), its distance from its initial mark, as measured on its graduated scale will, however, fall short of the correct zenith-distance of that mark or arc of revolution;—the discrepancy augmenting with the inclination of the ring to the circle described by the bubble of the sphere. When the inclination equals 90°, in which case (the plane of) the ring passes through (the centre of) the sphere in the direction of its axis, the arc of revolution may amount to 90°, without displacing the (unserviceable) bubble of the ring from between its marks *.

Let $a\,b'\,a'b$ be the great circle of the sphere parallel to the horizon; $b\,b'$ the vertical circle perpendicular to the axis of rotation $a\,a'$, described by the bubble of the sphere now at rest at the vertex v; and let v' be the bubble of the (oblique circle, or) ring $r\,r'$, inclined to $b\,b$ at an angle equal to $v\,m\,v'$, m being their point of intersection or initial mark where the bubbles of the ring and sphere coincided when in the same vertical.



Then as the bubble of the ring will be stationary at that point of the ring the most elevated above the horizontal circle, v' will be equidistant, or 90°, from r and r', and also touch the nearest of the small circles of equal altitude described round v as a centre, so that $m\,v'\,v$ must be right-angled at v'. We shall, therefore, have given in the right-angled spherical triangle $v\,m\,v'$ the leg $v'\,m$ (or zenith distance of m as given by the bubble and graduations of the ring) and the angle (of inclination of the ring to the vertical circle)

* The bubble will nevertheless pass over an arc of 90° of the minute circle on which we measure the interior diameter of the ring.

circle) $v\ m\ v'$, to find the hypothenuse $m\ v$ (or true zenith-distance of the mark m).

Were fluids indeed subjected, as we have hitherto supposed, to the *sole* action of gravity, our explanation of the theory of the spirit-level might be considered as complete; but from the effect of the mutual attraction of glass and the liquid of the level, the figure &c. of the bubble, as we shall proceed to demonstrate, must suffer material alterations.

[To be continued.]

LIV. *Observations on the Crystalline Form, &c. of the Gaylussite.* By W. Phillips, *F.L.S. G.S. &c.**

IN the *Ann. de Chim.* for March 1826, is inserted an account and analysis of a mineral newly discovered in a natron-lake in Colombia, by M. Boussingault, followed by a description of its crystalline forms, by M. Cordier. It appears to be a hydrous carbonate of lime and soda; consisting of Carbonate of soda 33·96, Carbonate of lime 31·39, Water 32·20, Carbonic acid 1·45, and Alumine 1·0, according to M. Boussingault. It has received the name of Gaylussite, in honour of the celebrated French chemist M. Gay-Lussac.

Five crystals of this substance have been presented to me by my brother, who lately received them from Robert Stephenson, a gentleman connected with the establishment of the Columbian Mining Company. One only of these crystals is what may be termed symmetrical in its form, the rest being elongated and channelled on their surfaces in a very remarkable manner. M. Cordier also appears to have possessed only one regularly-formed crystal; but as this was not, as he observes, sufficiently bright for the use of the reflective goniometer, he was compelled to rely on the common one for the measurement of its planes. Mine, on the contrary, is remarkably brilliant, and even transparent: I submitted it therefore to the former instrument, which confirms the most important measurements by M. Cordier.

The primary form adopted by M. Cordier is an irregular octohedron; but finding, as he observes, that "it is not easy to make it clearly appear how the planes of these crystals relate to that form as their primary," he has substituted, "as being more simple, and as Haüy had done in analogous cases, an oblique prism." The measurements and cleavages of these crystals have led me to the conclusion, that the primary is in reality an oblique rhombic prism, but of different measurements

* Communicated by the Author.

ments to that of M. Cordier, and altogether constituted of different planes; that which is adopted by that gentleman does not coincide with the cleavages of the mineral, while that which I propose is bounded by them: the terminal planes decline from one acute angle of the prism to the other.

The planes *c c'* of the following figures have been adopted by M. Cordier as the lateral planes of his primary prism, and the plane *c* (if I understand his statement correctly), as the terminal plane.—Slight, but very uncertain indications of cleavage exists in the direction of the latter, but none parallel to the former; while cleavages parallel to the planes M M are easily obtained, and of uncommon brilliancy; and parallel to P, I have obtained a cleavage sufficiently bright for the use of the reflective goniometer.

Fig. 1.

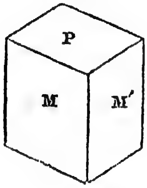


Fig. 2.

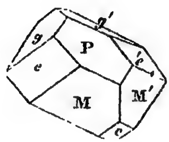
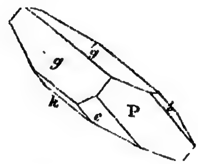


Fig. 3.



Measurements by the reflective Goniometer.

M on M on cleavage planes } . .	68°	50′	M on *e*	137°	45′
			——— *g*	110	10
P on M or M′	96	30	——— *k*	145	35
. .	49	55	*e* on *e′*	70	30
c or *c′*	125	10	——— *g*	152	20
g or *g′*	136	32	——— *k*	144	46
k . .	90	5	*g* on *g′*	110	30
M on *c*	110	20	——— *k*	124	30

The crystals do not occur in the determinate form of fig. 1, but are generally elongated, owing to the increased dimension of the planes *g, g*, thus greatly reducing the planes M M′, or annihilating them, as in fig. 2: the crystals are often still further elongated, by narrow and repeated alternations of portions of the plane *e e′*, and *g, g′*, thus giving them the effect of being deeply grooved, or channelled.

As no description of this mineral has yet appeared, as I believe, in any one of our Journals, I subjoin the following, chiefly extracted from the accounts given by MM. Boussingault and Cordier.

It occurs in detached crystals, disseminated in clay; the less perfect of them might readily be mistaken for crystals of selenite,—the more perfect and smooth have more nearly the aspect

aspect of calcareous spar: the latter are colourless and transparent, and are doubly refractive in a high degree: in respect of hardness, this substance is between the two above mentioned. Spec. grav. 1·928, 1·950; but that of the remarkably brilliant and solid crystal above figured, was found by my friend S. L. Kent to be 1·990. It is extremely brittle; is easily reduced to a grayish powder; the cross fracture is conchoidal, and the surfaces produced by it are of a vitreous lustre. The crystals are neither phosphorescent by friction, nor electric by heat; nor does any phosphorescence appear if the powder be thrown on a live coal. When exposed to heat in a matrass, it decrepitates slightly and becomes opaque: decrepitation continues until it has acquired a red heat; if then subjected to the flame of the blowpipe, it melts rapidly into an opaque globule, which once formed, is infusible; and which if placed on the tongue when it is cold, has a decidedly alkaline taste. In nitric acid it dissolves with brisk effervescence, and if then left to spontaneous evaporation, fine crystals of nitrate of soda are formed, floating in a solution of nitrate of lime.

This mineral is found in great abundance near Lagunilla, a little Indian village, situated one day's journey S.W. of the city of Merida. It occurs disseminated at the bottom of a small lake in a bed of clay covering carbonate of soda, termed by the natives *urao*, which has been described by M. Palacio Faxar, in a Memoir inserted in the first volume of the Institution Journal. The natives term the crystals of Gaylussite *clavos* (nails), from their general form, doubtless, when greatly elongated. A specimen of the *urao* was likewise received by my brother. It occurs in long slender crystals, which are very indeterminate and dull, but affording one bright cleavage parallel to their axis: they radiate from a common centre.

The following particulars respecting the relative positions of the Gaylussite and Urao are extracted from the letter received by my brother with the specimens.

The lake of Laguilla (Lagunilla, Boussingault; Lalagunilla, Faxar) is about two days' journey from the southern extremity of the lake of Maracaibo: it seldom exceeds six feet in depth: the water reposes on a stratum of black slimy mud in which the crystals of *Gaylussite* are disseminated. Below the mud, which varies from 18 inches to two feet or upwards in thickness, appears the upper layer of *urao*, confusedly crystallized, and varying from two to four inches in thickness. It is extracted by expert divers, who can remain a long time under water; they guide themselves, when diving, by a long pole which they stick into the mud, where they expect to be suc-

successful. The divers told me that there are other and inferior strata of urao and mud alternating to a depth which they could only reach by extraordinary exertions. The urao is used by the natives to give pungency to their tobacco, by steeping it in a solution. Smoking cigars thus prepared produces soreness of the mouth, really amounting to a slight salivation. The inhabitants in the vicinity of the lake use a preparation of this salt worked into a paste, with tobacco, and which they call *chimo*, carrying it in a small box slung round the neck, and occasionally rubbing the nauseous mixture upon the gums and tongue,—a practice which appears to be of Indian origin. According to the analysis of Boussingault, this salt differs in no respect from that of natron.

M. Palacio Faxar says (R. I. J. vol. vi. p. 192) that the urao was analysed by Gay-Lussac, who found it to be natron in no respect different from that found in the lakes of Egypt and Fezzan.

LV. *New Phænomena caused by the Effect of Magnetic and Electric Influence, and Suggestions for ascertaining the Extent of the Terrestrial Magnetic Atmosphere.** *By* J. H. Abraham, *F.L.S.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

THE subject of this paper is, generally speaking, one that has been till lately less understood than any other in natural philosophy.

It is a branch of science of which I have attempted to gain some knowledge by numerous and often repeated experiments. It is a branch of science so intricate in its laws and subtile in its effects, that we can make but little progress in it without experiment; and as it is a subject which has of late excited intense interest, it is presumed that the following observations and suggestions may not be deemed unworthy of insertion in your valuable Journal.

Active magnetism may be communicated to, or more correctly concentrated in, a bar of steel of any form, by rubbing one of its sides only, and the power will be found to be equal on any part of its surface at the same distance from the equator of the bar or magnet.

* Communicated by the Author. Part of this paper has been read before the Royal Society; and the whole was read before the Sheffield Literary and Philosophical Society in August 1826.

Magnetism,

Magnetism, like electricity, may be rendered active on every part of the surface of a body (the centre excepted), though the stimulating power be applied to one of its sides only. But they differ very materially in other respects: If we communicate the electric fluid to a Leyden jar, and a second be connected with it by a conducting medium, they both will become equally electrical, and that instantaneously. But in magnetism this rapid transmission of the fluid from one bar to another, does not take place: if two unmagnetized bars of steel be placed longitudinally in contact, or even one upon the other, and a set of bars be carried over the uppermost, it will become strongly magnetical; but the lower bar, if removed immediately, will not have received power sufficient to attract the finest needle.

While repeating several of my experiments on the similarity in some respects, and the dissimilarity in others, between the electric and the magnetic fluids, I was led to believe that a more perfect conductor for securing buildings from the effects of atmospherical electricity might be produced than any hitherto used. For the performance of the experiment I procured two cast-steel rods properly hardened and tempered, each three feet in length, and half an inch in diameter; one end of each bar must be hammered to a fine point.

In order to prove the superiority of a point over a knob, and a pointed magnetic conductor over one nonmagnetic, I conducted the experiment in the following manner:—

I placed a brass ball two inches in diameter upon a stand, at the distance of one inch from the prime conductor of the electrical machine; which in this experiment represents a positively electrified cloud. When the machine was put in motion, a stream of the electric fluid passed from the prime conductor to the brass ball; which may represent a building or other object (if not in a negative state of electricity) in a minus state compared with the prime conductor. Upon presenting the unmagnetized rod to the prime conductor when the machine was excited, it was robbed of the electric fluid as rapidly as it was produced, at the distance of nine inches, so as to deprive it of the power of passing a spark to the brass ball. The magnetic rod produced the same effect at the distance of 12 inches from the prime conductor; consequently we may fairly presume that a steel rod rendered powerfully magnetic, will secure a building, in every direction, to a much greater extent than one that is not magnetical.

M. Gay-Lussac (in a paper in the *Ann. de Chimie*, vol. xxix. p. 105, "On the length of the electric flash producing lightning,")

ning,") observes, that "when lightning falls on a lightning-rod, it frequently happens that a small portion of the point is fused; and this effect is not very different to what may be produced by large electrical batteries." Hence we presume that a pointed rod which has been a long time erected, may have lost much of its original conducting power, owing to its pointed end having become oxidated from electrical and other atmospherical causes, and consequently become knobbed or rounded at the upper end; and as the safety of the building depends upon the *silent and rapid* transmission of the electric fluid into the earth, particularly when the charge of the descending fluid is great, I should recommend that the upper part of all conductors of lightning be made of steel properly hardened and tempered, to hold concentrated at the point the greatest magnetic power that can be given to the rod; and well gilt at the point, to preserve it from the action of the atmosphere. This conductor, from the preceding experiment, would receive the approaching accumulated electrical fluid at a much greater distance than an equally fine nonmagnetical rod; whereby its discharging power upon that conductor would be greatly diminished, and the building rendered more safe from the effect of the destructive element.

The 42nd Number of the Journal of Science and the Arts, contains an interesting paper, by Lieut. Johnson, R.N., "On local and electrical influences on compasses."

After having attentively read the paper, I felt so much interest in several of the experiments, that I was induced to try whether those which the Lieutenant asserts "produced a variation of the needle in the compass-box, by wiping the glass cover with a silk handkerchief or other soft substance," were correct. In the course of these experiments I noticed several phænomena which the Lieutenant seems not to have been aware of at the time his paper was written. He remarks, "that having observed a considerable deviation produced on the compass needle by the mere act of wiping the dust from the glass cover of the compass-box with a silk handkerchief, I rubbed it successively with silk, woollen, cotton, and linen, and found that they produced similar results, and also leather in a less degree, viz. causing a considerable deviation, *generally* to the *eastward*, sometimes as much as 20°, and once to 40°, from the magnetic meridian." After repeating this experiment several times, I was not able to discover, when the compass needle was placed due north and south, and the box containing the needle was kept firmly in its place during the experiment, that the friction produced any variation whatever. He further observes, "that one pole of the needle adhered for

for more than a minute to the glass cover, and then gradually losing such power, it declined again to its horizontal and directive positions." This I invariably found to be the case when any *apparent variation* took place, which *may be produced* by giving the compass-box the slightest motion during the time that the glass cover is excited: this causes the needle to oscillate; and when the glass becomes sufficiently electrical, it attracts *one* point of the needle; and if that point be 20° or 40° on *either* side of the magnetic meridian, at the moment when it becomes stationary by the attraction of the fluids, it gives not *a real* but only *an apparent* variation of the needle. When the glass cover of the compass-box is rubbed with a silk handkerchief, positive electricity is produced upon its upper surface, consequently negative electricity will pervade the opposite surface; which, I observed, invariably attracted the *south pole* of the needle; and when left undisturbed, this mutual attractive force of the two fluids generally continues for the space of one minute.

Lieut. Johnson likewise states, "that the rubber (the silk handkerchief), and various other substances, as the metals, &c. when presented to the glass cover, have the power and property of repelling both poles of the needle." This effect I have reason to doubt, as I have not been able to produce it in a single instance. No small substance which I yet have tried that is not magnetical, with the exception of the finger (*a curious fact*), will immediately neutralize the effect produced by the attraction of these mysterious and powerful agents.

A bar of warm or hot iron has not the same effect as the finger, in immediately neutralizing the attractive power of the two fluids; but when the needle by repulsion, or saturation by contact, recedes from the glass plate, the austral pole will be attracted and the boreal repelled on having a bar of hot iron presented to them. Being anxious to ascertain whether the boreal fluid and positive electricity are similar in their effect to what was witnessed with the austral fluid and negative electricity, I adopted the following experiment:

Having charged a Leyden jar positively, I placed a horizontal needle upon a stand, so as to be on the same plane with the knob of the jar, and within the influence of the electrical atmosphere. The north pole, as I anticipated, was instantly influenced and drawn from the magnetic meridian; and it pointed to the brass knob of the jar, which was situated due west, or in the line of the magnetic equator. The needle was removed from the stand and placed on the table, about five or six inches from the jar, and the south pole was immediately attracted to the negative side of the jar, confirming what I

have

have long anticipated,—almost an identity of these two extraordinary fluids. Every natural and artificial magnet is surrounded by a magnetic atmosphere; consequently the "great globe itself" must be encompassed by an attractive power, which decreases in an unknown ratio, as its extent from the earth's surface increases. If the particles composing the magnetic atmosphere of an artificial magnet be sufficiently subtile to penetrate a block of marble or a stone wall of half a yard in thickness, so as to disturb the repose of a magnetic needle on the opposite side of the block or wall (which is easily proved to be the case, by a bar magnet and a delicate needle), and on removing the interposing body, if the needle be affected by the atmosphere of the same magnet at a greater distance (which is a fact), it proves that free space is necessary for the magnetic body to act with full force upon any object within the sphere of its attraction.

If an atmosphere or influencing power extend to the distance of 50 or 60 inches from the poles of a small bar of steel rendered powerfully magnetical by artificial means, to what distance must the polar influence of the terrestrial magnet extend?

Were we in the possession of the ratio in which the magnetic power decreases in either a natural or an artificial magnet of a *certain power*, we ought likewise to have a knowledge of that force or power in the magnet submitted to experiment, to enable us to calculate its action upon other substances at any given distance from its point of greatest force of attraction or repulsion, and likewise the properties of the body experimented upon.

In the works which I have read upon this interesting subject, I do not recollect having met with a theory which would enable us to ascertain the extent of the magnetic influence of our globe in any given latitude. Professor Poisson remarks, that "the magnetic power of the earth, like that of all other magnets, is the product of two factors; one of which depends upon the distribution of the two fluids, the boreal and the austral in its interior; and the other, common to all substances capable of magnetization, expresses the intensity of attraction and repulsion at a unity of distance, and between quantities of fluid also taken as unity. It may therefore vary for two different reasons; because the particular magnetic state of the terrestrial spheroid has changed, or because the mutual action of the particles of the magnetic fluid weakens or strengthens in all substances capable of retaining magnetism."

A knowledge of the ratio and extent of the magnetic atmosphere of our earth in any latitude, appears to me so necessary

cessary and desirable, (if it can possibly be obtained,) that I embrace this opportunity of suggesting what I consider the most likely means of ascertaining this important object.

I recommend the employment of a machine that has not hitherto been of the least use in promoting a knowledge of either the arts or sciences, although numerous adventurous individuals have perished in the attempt to navigate it through the aërial flood, to gratify the idle curiosity of countless multitudes.

If the dip be taken at the place from whence the balloon is intended to ascend, and the same needle (which ought to be of the most perfect construction) be carefully deposited in the car attached to the balloon, any number of observations may be recorded by the aëronaut or his companion, according to the variation or decrease of the dip as the altitude of the observers increases. It will be necessary, in order to insure the accuracy of the experiment, to have a correct set of apparatus, independent of the dipping-needle; as a mountain barometer, a thermometer, &c. to ascertain the altitude and the temperature of the air at the moment when the dip is taken at each observation.

If during an aërial voyage the experimenters (for I consider one person incapable of managing the balloon and making the necessary observations) should be elevated only two or three miles, they, I have not the least doubt, would observe a diminution of magnetic action upon the needle, long before they reached that elevation; or its influence will extend far beyond our atmosphere: and if the distance of two or three miles from the earth's surface would only afford us two or three very minute and progressive variations in the dip, we might be enabled thereby to solve many curious problems in magnetism.*

Holy Green House, Sheffield, Feb. 9, 1827.

LVI. *Description of New Succulent Plants.* *By* A. H. Haworth, *Esq. F.L.S. &c.*

In this my ninth Decade of new Succulent Plants, are described ten South-African species; the first five of which were discovered near the Cape of Good Hope, and sent from thence to the royal gardens of Kew, by Mr. Bowie, where

* An experiment on this subject in which the dip appeared to be rever ed at the elevation of about 8000 feet, was made by Sacharof and Robertson during their aërial voyage from St. Petersburgh, on the 30th of January 1804. See Phil. Mag. vol. xxi. p. 199.—Edit.

they

they are all now flourishing. They appear to belong to the Linnæan genus *Cotyledon;* at least as it now stands.

They all likewise appear, thus far at least, to be unrecorded species; and belong to two very distinct divisions of the genus, the last of which, PARVIFLORÆ, will hereafter, from its included unequal *stamina*, and furfuraceous appearances, become the type of a *new* genus: when we shall become sufficiently acquainted with its component species and fructification; and I propose for it the name PITUREA, *à voce* πιτυρον *furfur*.

In all probability also, *Cotyledon coccinea* of Cavanilles will be the type of another new genus; and the species allied to *C. umbilicus*, that of a third. But these speculations must at present be relinquished, for want of sufficient specimens.

The species of the first *Section* of *Cotyledon*, in the present *Decade*, are nearly all, very stately plants, with showy terminal dichotomously cymed bunches of large pendulous flowers, of a deep aurora colour, approaching to scarlet, with exerted equal *stamina*, and are produced every summer upon old plants. The species of this division too, are easily propagated from cuttings in the usual way; love sandy earth, and will thrive in any good greenhouse: and indeed, many of them make a fine appearance there; being conspicuous ornaments even when out of bloom, through the great contrast formed by their broad mealy leaves, with the more ordinary foliage of every greenhouse.

And the section of the genus PARVIFLORÆ, hereunder further explained, and which I have above proposed to call PITUREA, is very interesting, in a philosophical point of view, because some at least of its component species have the remarkable faculty of gradually by day-light opening, and even retrofracting their blossoms, and of again closing them, in the same day; and this for several days successively.

Nor are these plants all, which may hereafter recede generically from the present genus *Cotyledon*, many recorded species of which are at present but little understood.

With respect to the remaining plants which complete this decade; one is a new *Gasteria*, and the remaining three belong to the succulent aphyllous division of the genus *Euphorbia* of Linnæus; and were also discovered in South Africa, by Mr. Bowie, and are now flourishing in the greenhouses of the royal gardens at Kew, where I have made the following descriptions of them.

Chelsea, Dec. 7, 1826. A. H. HAWORTH.

Decas

Decas nona Plantarum Novarum Succulentarum.

Classis et Ordo. DECANDRIA PENTAGYNIA.

Genus, COTYLEDON *Linn. &c.*

Sectio, GRANDIFLORÆ, *Inflorescentia* altè pedunculata, *floribus* cymoso-umbellatis terminalibus, plantæ ramos superbè superantibus; *corollis* monopetalis quinquefidis magnis (inter affines) campanulatis, pallidè coccineis, apicem versus revolutis. *Caules* grossi suffruticosi, *foliis* carnosis crassis, sæpissimè magnis farinoso-albis, obtusis cum mucronulo, aëre aperto margine roseo, sive purpurascente.

Obs. Post florescentiam (ni malè memini) *Flores* reerecti sunt.

crassifolia. C. (thick mealy wedge-leaved) farinoso-alba:
1. subsimplex: foliis rhombeo-obcuneatis incrassatis.

Habitat C. B. S.

Obs. Frutex, nunc sesquipedalis, erectus succulentus, *ramis* perpaucis crassis. *Folia* subdistantia decussata opposita, omnium *Cotyl.* cognitarum crassiora, valdè farinosa; supra medium marginibus fusco-purpureis. *Flores* non vidi.

Vigebat in regio horto Kewensi ante A.D. 1824. G. H. ♄.

Pone *Cotyl. oblongam* locarem.

viridis. C. (simple, green-leaved) foliis obovato-cuneatis
2. perviridibus, caudice valdè cicatricato.

Habitat C. B. S.

Obs. Bipedalis (tertio anno) erecta, caule caudiceve ferè simplici, foliorum vestigiis maximè cicatricato; cicatricibus lunulæformibus, lunulis obtusissimis, pallidis, dorso jacentibus; magísque quàm in aliis cognitis *Cotyledonibus* profundioribus et conspicuioribus. *Folia* mediocria; macra (inter affines) et semper viridia. Cum prioribus sine floribus vigebat ante A.D. 1824. G. H. ♄.

Pone præcedentem locarem.

** PARVIFLORÆ, *floribus* parvis erectis albis roseo-striatis sæpè spicatis; foliis subfurfuraceo-punctatis.

rotundifolia. C. (dwarf round-leaved) foliisc rectis confertis
3. rotundatis sordidè viridibus, ramis brevibus decumbentibus.

Habitat C. B. S.

Obs. *Suffrutex* pygmæus non adhuc semipedalis; ramis sæpè humum versus depressis, vel erectioribus, carnosis. *Folia* plana, subtus convexa, et præcipuè per lentem undique farinoso, crebrè punctata, punctis rotundis minutissimis parùm regularibus; marginibus (foliorum) minutè cartilagineis sine punctulis.

Cotyl. hæmisphericæ affinis, at foliis duplò latioribus, duplóque tenuioribus: ramis minùs erectis, brevioribus, caudice crassiori. *Flores* non vidi. G. H. ♄.

cristata. 4. C. (Coxcomb-leaved) foliis petiolatis cuneato-triangularibus, apice crispo-cristatis.

Habitat C. B. S.

Florebat copiosè in regio horto Kewensi, Sept. 1826. G. H. ♃.

Descriptio. *Herba* succulenta foliosa sempervirens, caudice brevi crasso. *Surculi* ramulive perbreves, pilis ramentiformibus rufis respicientibus sive deflexis, densissimè vestiti. *Folia* numerosa erecta subuncialia, sordidè viridia, obsoletè punctatim furfuraceo-puberula crassa, sive pulvinatim tumescentia, apice purpurascentia, sed deorsùm in *petiolos* teretes clavatos (folio breviores) pedetentim abeuntia. *Flores* (in *spicas* terminales erectas flexuosas graciles,) parvi sessiles quoque erecti, et horizontaliter aperientes ante horam octavam A.M. atque apud meridiem gradatim usque ad spicam ipsam retroflectentim arctè adpressi: sed vesperam versus sensim sensimque retrogressi; necnon post solis occasum arctim omninò reclausi, ut ante mirabilem aperientiam: et in hoc more per varios dies!

clavifolia. 5. C. (club-leaved branny) foliis petiolatis claviformibus incurvantibus, apice subcrispo acuminulato.

Habitat C. B. S.

Florebat cum præcedente in Sept. 1826. G. H. ♄.

Descriptio. Priori valdè affinis at abundè distincta videtur. *Folia* subtrientalia, plusquam duplò angustiora, *petiolo* magìs incurvo, vix puberula, ramentis caulinis fortè paucioribus; cum eodem modo florendi; at *flores* duplò majores, sæpè binati vel ternati: *corolla* tubo subincurvo robustiore, viridi; laciniis intùs albis, extùs (uno latere) purpureis, et basi undato-sublobulatis ut in priore.

Obs. Sequens est (ni malè memini) alteram descriptionem (alio tempore factam) hujus speciei; vel si non, ultimæ

ultimæ *Cotyledonis.* *Corolla* tubo longo crasso angulatim cylindrico. *Stamina* inclusa, filamenta decem recta alba, horum quinque tubo ¼ breviora, eoque usque ad medium adnata: quinque alia alternantia, tubi longitudine, eoque usque ad medium, altiùs adnata. *Antheræ* pollinosæ, flavæ. *Germina* quinque tubo parùm breviora virescentia, cum continuantibus *stylis* parùm subulata, *stigmatibus* obtusis inconspicuis. *Squamula* germinis ordinaria subrotundato-quadrata retusa, atque hyalina.

Classis et Ordo. Hexandria Monogynia.

Gasteria, *Duval.*—et *Nob.* in Phil. Mag. Oct. 1825.—*Synops. Pl. Succ. &c.*

bicolor. G. (half-marbled, lightest green) foliis angustè lin-
6. guiformibus obtusis biconvexis lævissimis pallidis imis, subtùs maculato-marmorescentibus.

Obs. *Folia* inter erectiores, nunc pedalia disticha ecarinata, sed fortè non in ætate; omnium pallidissimè virescentia, mucronata, suprà immunia; marginibus supernè cartilagineo-asperiusculis, et intra ipsam marginem margine aliâ lineari concinnâ atro-viridi. *Subtus*, infima *folia* crebrè ac subsordidè et saturantèr variè marmorescentia. *Flores* non vidi.

Pone *Gasteriam candicantem* Nob. *Revis. Pl. Succ.* 46. sive *G. ensifoliam* Nob. in Phil. Mag. in loco suprà citato locarem; quæ ambæ *Gasteriæ* nunc carinantibus foliis gaudent.

Classis et Ordo. Dodecandria Trigynia.

Euphorbia *Auctorum.*

Sectio, Aculeatæ, *ramis* crassissimis nudis angulatis; angulis spinosis; *foliis* minutissimis, citiùs marcescenti-deciduis seu caducis, in summis ramorum solùm (cum floribus ordinariis) visis, et subindè ferè (è parvitate) invisibilibus, sine lente.

Subsectio, Florispinæ, spinis solitariis floriferis.

stellæspina. E. (starry-spined) multangularis: valida: sin-
7. gulis spinis ramoso-stellantibus rufescentibus; mortuis nigris.

Habitat C. B. S. G. H. ♄.

Flores fortè affinium; non examinavi. G. H. ♄.

Descriptio. *Planta* in regio horto Kewensi (Oct. 1825)

1825) dodrantalis est; erecta sub-12-angularis, tres uncias crassa; *spinis* infra *foliola* minuta ordinaria trilinearia lineari-lanceolata vix lineam lata utrinque attenuata glaucescentia. *Spinæ* quinquelineares expansæ ramulosæ validæ, ramulis (spinarum) duobus alternis, quatuorque aliis subradianter patentibus.

Obs. Distinctissima et præsingularis species. Inter affines multangulares et pone *E. polygonam* Nob. locarem.

Subsectio, STERILISPINÆ, spinis sterilibus.

cærulescens. E. (square blue Cape) articulatim interrupta:
8. erecta: tetragona: ramis basilaribus luridè cærulescentibus.

Habitat C. B. S. G. H. ♄.

Obs. Nunc tertio anno, in regio horto Kewensi, subbipedalis est; *ramis* à radicali base grossa, simplicibus; *spinis* marginalibus, affinium modo digestis, sive oppositè geminatim divaricantibus atro-rufis semuncialibus. *Flores* ut in affinibus sine dubio; at non examinavi.

Obs. E. canariensi valdè affinis, at magìs articulata, longissimè humilior et duplò gracilior; ramorum subcæruleorum articulis 1—4-uncialibus solùm, spinis quàm in *E. canariensi* duplò longioribus: nec 10—20-pedalis, ramis ramulosis 4—5-angularibus longissimè continuantibus viridibus, cicatricibus annuis annularibus vix impressis solùm notatis, ut in *E. canariensi.* Nihilominùs pone eam locarem, cui simillima.

tetragona. E. (slender square light-green) subsimplex: erecta:
9. caulibus subgracilibus continuosis lætè viridibus; spinis patentibus geminatis.

Obs. Nullæ valdè affinis. Nunc subtripedalis firma erecta tetraquetra. Ultimæ affinis at altior, et plusquam duplò triplòve gracilior, spinis minoribus, et valdè distincta. Ambas hasce præsucculentas plantas sine dubio post *E. canariensem* collocarem.

squarrosa. E. (the Chevaux-de-frise) tuberoso-strumosa:
10. ramis simplicibus decumbentibus squarrosè spinoso-pinnatisectis.

Obs. Affinis *E. procumbenti* Meerburg, *Rariores, t.*55. *E. uncinata* DeCandolle. *Radix* strumoso-tuberosa, 2—3 uncias longa. *Rami* capitati pervirides, sive è capite tuberis circulariter erumpentes cæspitosè patentes bilaterati, subsemipedales planiusculi; (*subtus* convexi)

vexi) torquati et quasi pinnatisecti, è spinis geminatis patulis rufo-fuscis marginalibus brevibus insuper pedunculos productos carnosos crassos trilineares, obliquè spiralitèr tortos et squarrosè sexfarios insidentibus; et quasi in apice ramorum in totidem angulis.

Folia ordinaria affinium, in ramorum apicibus habet minutissima subrotundo-cordata, ferè invisibilia citiùs marcescentia, et caduca.

Obs. Plantam hanc mirabilem in propriâ subsectione locarem cum *E. procumbente* Meerburg (quæ est *E. uncinata* DeCandolle, ut suprà:) necnon *E. scolopendra* Nob. in *Synops. Pl. Succ. p.* 126; quæ ultima nunquam cum radice tuberosâ, neque ramis numerosis simplicibus ambientibus vidi: sed cum ramis solitariis, ramuliferis, et duplò majoribus, magísque dilatatim obliquè pallidéque venosis quàm in *E. procumbente.*

LVII. *On the Geology of East Norfolk; with Remarks upon the Hypothesis of Mr.* Robberds, *respecting the former Level of the German Ocean. By* R. C. Taylor, *Esq. F.G.S.*

[With Engravings.]

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

THE district which is the subject of examination in Mr. Robberds's "Geological and Historical Observations on the Eastern Vallies of Norfolk," noticed in your last Number, having particularly occupied my attention, a perusal of the work has induced me to send you some remarks on this inquiry, and on the validity of the conclusions which the ingenious author has adopted.

Mr. Robberds shows that these valleys, which are now for the most part solid and productive land, yielding rich pasturage to many thousand head of cattle, were "at no very distant period, arms of the sea, navigated by our forefathers."

The proofs of this change are arranged under two heads: Physical and Historical.

Under the first class are enumerated the connection between the valleys and the German Ocean; the resemblance which their outline bears to the forms generally exhibited by æstuaries and inlets of the sea; and the remains of marine shells and exuviæ discoverable along their margins, at the elevation of 40 feet. These beds of shells are stated to have the following striking and peculiar characters.

"1st. None of them, except a few casual specimens, belong to any

any extinct or even rare species; but they consist entirely of the littoral shells, which now abound in the German Ocean, and are constantly met with on its shores or in the æstuaries into which its tidal waters flow."

"2d. Many of them, particularly the *Buccina*, are still very perfect, and in excellent preservation. 3d. The beds are found at various places, and uniformly at the same height of 40 feet. 4th. They appear, in most instances, not to extend beyond the face of the hills. 5th. They are mixed with vertebræ of small fish, and bones of land animals, decayed vegetable substances resembling *fuci*, fragments of coal, &c.

From these physical circumstances the following conclusions are drawn:—

1st. The shells found either below the soil that fills these basins, or on the sides of the surrounding hills, are unquestionably marine; they were *therefore* deposited by the waters of the sea.

2d. They contain no exuviæ that are peculiar to the older strata, but all resemble those of the testaceous mollusca now found in the neighbouring ocean; *therefore* the sea, by whose waters these deposits were formed, was the German Ocean.

3d. These beds of shells and other coincident traces of an ancient beach are found about 40 feet above the present surface of the valley of the Yare; *therefore* the waters of the German Ocean once flowed up, and permanently occupied this valley at that elevation.

4th. The valleys of the Bure and Waveney are upon the same level, and communicate with that of the Yare; *therefore* they were at some period connected branches of an extensive æstuary filled by the waters of the German Ocean, to that height at which the traces of their residence may still be discerned.

Historical proofs.—The ancient map deposited in the Yarmouth town chest, of which document an inaccurate copy was published in Ives's *Garianonum*, indicates that many centuries ago, there prevailed a confused notion that these valleys were in earlier times filled by the waters of the sea. Imperfect as is this testimony, it derives confirmation from the remains of anchors which have been discovered in the marshes; evincing the spots where they were found to have been permeable to maritime vessels, since the art of navigation has been known to man. There is further evidence in the sites of Roman forts, most of which the author conjectures, "were built for the defence of this very exposed part of the Saxon shore, against the inroads of those formidable Northern pirates by whom it was afterwards so frequently laid waste." Caister near Yarmouth, and Burgh Castle, generally viewed as the true *Garianonum*,

anonum, appear to have been the principal frontier posts; and the names and situations of Wheatacre Burgh, Happisburgh, Smalburgh, and another Burgh in West Flegg, seem to refer all these places to a similar origin*.

A dissertation follows on the etymology of the Garienis. Mr. Robberds judiciously maintains, that there are no traces of a *Latin* origin in the term, as applied either to the individual river now exclusively bearing the name of Yare, or to the several openings by which this large inlet is connected with the sea; the most probable origin of the Garienis being the Celtic *Garu*, and thence *Garu-an* (the rough river). In addition to the instances of the Yarrow and the Garonne, recited to prove the prevalence of this name, as applied to rivers in different countries, and under various modifications of language, may be mentioned the names of two mountain streams, the *Garw* and the *Garan*, in Glamorganshire.

In the etymology of the names given by the Saxons to many parts of this district, Mr. Robberds perceives further proofs of the state in which they found it on their arrival. The insulated plots of rising ground, interspersed in the wide part of the valley of the Bure, are still called Holms, the Anglo-Saxon term signifying islands. "It is remarkable that the names of nearly all the villages in the Flegg hundreds terminate in *by*, which Mr. Robberds conjectures may be derived from the word *bight* or bay. When the valleys were filled with water, the marginal indentations and recesses would present the appearance of bays, and these sheltered coves would naturally be selected, by maritime adventurers like the Saxons, as the first places on which to fix their abodes. The villages having this termination are all situated adjoining these bights†.

At Kirkley, ten miles south of Yarmouth, was the ancient haven and inlet of the sea, communicating through Lake Lothing and Oulton Broad, with the wide valleys of the Wave-

* Mr. Robberds is inclined, on considering the position of Wheatacre Burgh. to designate it the *Garianonum* of the Romans. Hitherto, but from its name, there has not arisen the slightest circumstance indicative of a station of so much importance at this point. There are no traces of Roman works, nor does the site command the main entrance from the sea.

† In process of time, Mr. Robberds conceives, the *by* became synonymous with *dwelling*: which may account for the exceptions to the rule. There are six or seven of these unconformable localities in Norfolk and Suffolk, far removed from sea, marsh, or low valley. The numerous small bays in Lake Lothing, Oulton and other Broads, are provincially called *hams*. Of the forty parishes which skirt the edges of the marshes between Norwich and the sea at Kirkley, a district abounding in similar interior bays, one parish only terminates in *by*, four in *ley*, six in *ham*, and sixteen in *ton*.

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ney and Yare. The area which is circumscribed by these valleys and the sea, forming the hundred of Lothingland, still retains its name of *the Island.*

A.D. 1004, Sweyn, as the Saxon Chronicle states, "came with his fleet to Norwich," which he plundered and burned. From the circumstance of this fleet proceeding so far as thirty miles into the interior, it is inferred that this could not have been effected in safety within the ordinary channel of the Yare, but that the whole valley was at that time navigable.

Domesday-book is the next historical document which supplies certain proofs of the sea having entered into the eastern valleys. These proofs exist in the *salinæ*, or saltworks, which are enumerated in many parishes, now distant from the ocean.

The bank on which Yarmouth is placed became firm and habitable ground about the year 1008; but it continued an island, that is, it had a northern as well as a southern channel, as late as the year 1347. From a memorial of the inhabitants it appears, that at the latter period, the main passage at Grubb's haven was silted up; that thousands of acres in consequence of the exclusion of the tide had become good land; and that the inland waters with extreme difficulty forced their way to the sea, through the opposing beds of sand and shingle, almost as far southward as Lowestoft.

In the 13th and 14th centuries, the contentions between the citizens of Norwich and the burgesses of the rival port of Yarmouth, gave rise to certain documents, which are useful in the present inquiry, by showing that to these periods trading vessels sailed up to Norwich, "*the King's Port*; where all foreign merchants paid their customs." The citizens pleaded "that Norwich was a mercantile and trading town, and one of the royal cities of *England*, scituate on the banks of a water and arm of the sea, which extended from thence to the *main ocean*, upon which shipps, boats and other vessels have immemorially come to their market."—"and all this long before Yarmouth was in being, even when the place which that now stands upon, was main sea."

The foregoing recital contains the substance of the evidence adduced to show, "that the eastern valleys of Norfolk were formerly branches of a wide æstuary, and that their present rivers and lakes are the remains of that large body of water, by which their surface was overspread, even in times comparatively recent." After reviewing all these circumstances, the conclusion to which the author arrives is, "that the change here observed is the result of *a depression in the level of the German Ocean itself*, which is now at least forty feet below the height where there is evidence of its having been stationary

at

at some distant period. In summing up, Mr. Robberds conceives that the sea once filled the interior valleys to the height of 40 feet, as marked by the ancient shells;—that the tides flowed at an elevation of 10 or 12 feet, at Burgh Castle, during the Roman occupation of that fort:—that at the Norman Conquest they were only about six feet high:—that there have been in every succeeding century, fewer and less extensive inundations of fresh-water in these valleys:—that old navigators observe within their remembrance, a sensible lowering of the waters in the present channels;—and that all these circumstances combine to mark "a progressive depression in the level of the adjacent sea."—"The rate at which this change has proceeded, might probably be calculated with mathematical precision; the data are rather uncertain, but they seem to indicate that the level of the sea has been regularly falling about eight or nine inches in every hundred years, which would carry back the period of its greatest elevation to about six thousand years ago."

Such are the inferences which the observation of Mr. Robberds has enabled him to form. He proposes hereafter to take a wider survey; to show that throughout this quarter of the globe, there are similar traces of the retiring waves, and that there is evidence of a corresponding elevation of the Indian and Pacific oceans.

In an inquiry of this nature, previously to assenting entirely to certain conclusions, it will be permitted me to scrutinize the "evidence of change," so near home; to examine the data which have led to this conviction, and formed the groundwork of the author's present arguments, and as may be concluded of his future reasoning.

Admitting at once the accuracy of the historical portion of this evidence, it still appears possible to demonstrate, that all the recorded and authenticated changes in the valleys under investigation, since the epoch of the deluge, were effected without any depression of the neighbouring ocean.

A slight preliminary sketch of this district may be useful. It includes ten principal valleys; all nearly upon the same level, approaching to one plane surface, having the outfall of their collected streams at Yarmouth; there passing with difficulty over the bar that obstructs the haven's mouth. Several minor ramifications, with numerous bays and indentations, almost incircle certain higher lands, forming greater or smaller promontories. Other spots of elevated ground, provincially termed Holms, rise, like islands, from the bosom of these marshy flats. The whole character of the scene is that of an arm of the sea, whence its waters have been withdrawn, or

whose bed has emerged from its original level. About 150 square miles, or one hundred thousand acres, are occupied by these valleys; and in traversing them, on the causeways by which they are now intersected, one is reminded of those similar but more extensive tracts of low lands on the opposite coast. The general width of the river Yare is about 150 feet; and of the Waveney and Bure 100 feet each. In their winding courses they frequently expand into or communicate with small lakes, locally termed Broads. There are upwards of sixty of these broads; besides as many smaller pools. Their depth varies from 15 to 30 feet, and they differ in extent, from one acre to 1200; that of Breydon being the first, and Lake Lothing the sixth in magnitude.

It may not be irrelevant to state, that the drainage of the greater portion of Norfolk and part of Suffolk, is effected by means of Yarmouth haven. The summit or highest edge of this area of drainage, extends to within seven miles of the sea at Snettisham, on the opposite side of the county. Within these limits is comprised an area of 1200 square miles in Norfolk, and 220 in Suffolk. Consequently, the quantity of water collected in, and passing out of, so extensive a basin is considerable. In connection with this subject is the geological fact, that these limits of drainage singularly correspond with the boundaries of the great deposit of clay, brick-earth and diluvial matter, resting upon the chalk, in this portion of Norfolk.

The most material circumstance now to be inquired into, is the presumed occupation of the eastern valleys, to the height of 40 feet, by marine waters, at an indefinite period, subsequent to the deluge. The authority for this supposition appears to be solely found in the beds of marine shells, which are exposed at that elevation, along the sides of the Norwich valley. These shells are described as strictly similar to those of the testaceous molluscæ now abounding in the adjacent sea, and continually met with upon our shores. Upon this apparently strict conformity to the genera of present times the conclusions of Mr. Robberds are obviously founded: but I conceive, if it should appear that these exuviæ are more properly assignable to an æra preceding the deluge which last affected our earth, that gentleman will consent to abandon the views he at present entertains.

It happens that a catalogue of the stratified shells of Bramerton, the principal source in this neighbourhood, whence Mr. Robberds, Mr. Sowerby, Mr. W. Smith, Mr. Leathes, and others have derived their specimens, was not long since communicated by me in the Geological Society's Transactions. From this list, which is capable of extension from subsequent discoveries

discoveries in the minuter genera of shells, naturalists are enabled readily to determine, whether such do in reality belong to the class of living shell-fish: or, if otherwise, what are the proportions it exhibits between those which appertain to the recent and the extinct species. Nearly the whole of the shells contained in this catalogue are accurately figured in Sowerby's Mineral Conchology, from specimens collected at this spot; and all are unquestionably similar to those which characterize, and are peculiar to the *crag*; or, as it is properly called, the upper marine formation. It is extremely probable, that upwards of one half of those enumerated have no recent analogues, and the practised eye of a skilful conchologist will detect varieties in many that appear to assimilate to those now living. At the same time it must be remembered, that they are associated with the remains of herbivorous animals, which have never been known in the present state of our globe,—the mastodon, the elephant, the gigantic elk, and the enormous horned bison. Their regular beds contain no works of art; no traces of the human species*. The British Museum contains a tooth of the mastodon, in which the enamel is converted into opal. This fine specimen is figured in Smith's "Strata Identified," and was discovered with horns of deer, at Whitlingham, in that same stratum of crag shells, described by Mr. Robberds.

In general, the fossils of this formation are not mineralized, but are very fragile. In Suffolk and part of Norfolk they are chiefly deposited in dry loose beds of sand, which are slightly consolidated and discoloured by iron. Thus at Languard Cottage, some curious artificial caverns have been formed in a thick bed of these shells. Where they occur in clay, they are partially mineralized; their surfaces are smooth, and sometimes glossy. At Whitlingham and upon the north-west coast of Norfolk, they are occasionally seen resting immediately upon the chalk, mixing with its flinty debris, and even having their cavities filled with chalk. Horns of stags are frequently found under the same circumstances, embedded in chalk marle, particularly along the sides of the Norwich valley, extending between Hellesdon and Cantley. In Essex, similar shells occur in a strong blue clay. Near Orford, in Suffolk, they are mixed with interesting varieties of coral and sponges, forming a soft porous rock used for building. In some other parts of its course this formation

* The rudely-shaped flint axes which have been discovered in the peaty bed of the Waveney Valley, are among the most ancient monuments of man in this island. They must be classed with the extraneous alluvial substances of our highest valleys.

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assumes the appearance of a gray sandstone; in others the shells are detected in an extremely hard clay-stone; but instances occasionally occur where organic remains are altogether wanting.

In remarking upon the absence, at Bramerton, of a particular shell, which is met with among the crag fossils of Harwich, Mr. Robberds considers that this circumstance alone is fatal to the opinion entertained by me, of the continuity of the formation. But it must be remarked, that it is characteristic of the shells and other organic bodies deposited with the crag; that they are by no means diffused in equal numbers and proportions throughout, as in some older strata; but occur at intervals, in groups and genera. Thus at Cromer the predominant and remarkable shells are *Mactræ;* at Runton, *Cardiæ;* nearer Cley, *Murex striatus;* at Bawdesey cliff, *Murex reversus,* and *Pectunculus;* at the Beacon, *Venus æqualis;* at Felix Stow, *Pectunculus* and *Voluta Lamberti;* south of Languard Cottage, *Murex contrarius, Cardiæ,* and *Mya lata:* at Bramerton and near Norwich are *Murex striatus, Tellinæ* and *Balani.* The absence, therefore, of one or of many species from any of these localities, cannot weaken the remaining concurrent evidences that identify this formation, nor can it lead to the confounding it with any other.

The shells of Bramerton, and other parts of the Norwich valley, consequently, belong to the crag formation, and are not an assemblage simply of the recent species which abound in our seas, although they are mixed with many that closely resemble the existing varieties.

Mr. William Smith, than whom a better practical authority on this question cannot be quoted, states that "through Norfolk the crag shells lie near to, or are in contact with the top of the chalk; and *under a loamy soil,* on or near some of the best land in Flegg and the Vale of Aylsham."

On the contrary, Mr. Robberds's experience goes to prove, —and without it his views of the flowing of the ocean tides through the Norwich valley at 40 feet elevation, cannot be sustained,—that the shells so far from being stratified, form only a beach or belt, in no case penetrating into the sides of the hills. That this may be partially and occasionally the case, it is by no means here intended to doubt, particularly after the positive investigation to which this point has been subjected. There are difficulties in discovering a vein of coal or a bed of iron-mine, even in the exposed face of a rock or mountain: how many impediments prevent the tracing any thin or soft stratum, in a highly cultivated country, thickly overspread with diluvial and alluvial substances! Norwich is nearly

nearly upon the western boundary of the regular deposition of crag: the few detached indications that are observed further in the interior valleys, seem rather to denote its antediluvian limits. Consistently with what is observed elsewhere, this deposit becomes thin and imperfect at its western edges, which have evidently been operated upon by diluvial agency. The high lands and the great accumulation of diluvium on each side of the principal valley, render examination difficult: the chalk itself rises above the level of the highest crag deposits; —the phænomena attendant on the sinking of deep wells are seldom observed or recorded; and it is chiefly on descending again into the other valleys of this district, that fresh proofs, more or less positive and abundant, present themselves.

Experienced well-sinkers, however, do affirm, that on forming deep wells, in various places around Norwich, at a distance from the river, they have occasionally encountered a stratum of shells overlying the chalk. In one instance, at a farm upon Mousehold, the depth perforated was 132 feet, of which the first 88 consisted of diluvial gravel and sand; then a bed, two feet thick, of conglomerated crag shells, consisting of *Murices*, *Cardiæ*, *Tellinæ*, and *Patellæ elongatæ*, immediately lying upon the chalk. Here, therefore, was absolute proof of a continuous shelly bed, extending beneath nearly 90 feet of diluvium to a considerable distance from its outcrop in the Norwich valley.

At Marsham, and in the adjacent vales, it is again discoverable, accompanied with many bones of animals, large vertebræ, and horns of deer.

Further north, at Aylsham, on sinking a well in 1824, at the depth of 60 feet, a bed four feet thick of crag shells, was met with. They consisted of the genera *Murex*, *Turbo*, *Natica*, *Mactra*, *Venus*, and *Tellina*.

On reaching the coast at Cromer, they are again observed at low water embedded in a sandy ferruginous stratum, resting upon the chalk. Along the whole line of this coast, extending from near Cley in Norfolk, to the Naze in Essex, in an extent of one hundred miles, this formation has been minutely and almost uninterruptedly traced, by myself. The result of this investigation has been fully confirmed by able geologists, in various portions of that district. (See Section II.)

The remains of certain animals have been so often observed accompanying the crag, that they may be considered as indicative of its extent even when other proofs are not attainable. Fragments of bones, teeth, skulls, and horns are repeatedly met with by the fishermen, when dredging for oysters at sea.

So

So abundant are these animal remains on the oyster banks opposite Happisburgh, that they are frequently drawn up in nets.

Further arguments in proof of identity and general continuity,—the two great facts which it was essential to establish, as applied to this formation and the theory of Mr. Robberds, —are, it is presumed, unnecessary. The supposed course of the crag formation across Norfolk and Suffolk is traced in Section III.

Some notices of the prevalence of fossil bones upon the eastern coast have appeared in the Philosophical Magazine, and Geological Transactions. To those enumerated might now be added many subsequent discoveries. I am happy in having been instrumental in attracting the attention of several observers to these phænomena. Since the year 1822, many interesting well-preserved specimens have been collected upon the beach between Winterton and Cromer, where heretofore they continued unregarded. It is useless to occupy these pages with a detail of localities; for in fact, traces are discernible at every mile. By far the greater number of specimens have been derived by means of the fishermen: a circumstance that confirms the opinion before given, that a considerable accumulation exists on some of the outer banks off this coast. Viewing these scattered fragments as the relics of those animals who once inhabited the surface of the upper marine formation, who roamed along the antediluvian shores and æstuaries, and fed amidst the forests of a former world,—how numerous are the proofs here assembled!

A detailed account of these deposits cannot but be instructive, and will be best supplied by those who have frequent opportunities for observation. There is some reason to hope that this information will be furnished by a reverend gentleman of East Norfolk, who possesses ample materials, and the ability to promote the science of which he is an admirer.

Let me be permitted to add here one word on the services which the establishment of a provincial museum at Norwich has already rendered, by furthering the progress of local geological discovery, by increasing the number of labourers in the field of science, and by furnishing a public depository of those interesting objects, which illustrate the structure and former condition of the surrounding district, and attest the revolutions to which it has been subjected.

In tracing the leading superficial features of East Anglia, it will be observed, that the general dip of the strata is towards the south-east, forming an angle of inclination, amounting probably to not less than 600 feet. At Harwich the upper

per surface of the chalk was reached at 64 feet *, and on the north-west coast, beginning at Hunstanton cliff, the inferior strata rise to the surface. The top of the chalk being sunk as much below the sea at Harwich as the bottom is elevated above the sea, on the north-western escarpment, and its entire thickness being estimated at about 400 feet, the slope will be somewhere as above suggested. This will be best understood by consulting the Section No. I.

It has before been remarked, that the principal drainage of Norfolk, comprised within that portion which is covered by diluvial clay and loam †, conforms to the slope of the chalk, and passes its collected waters to the sea at Yarmouth. In like manner the drainage water of nearly four-fifths of Suffolk, including the great clay district, conducted by several channels towards the south-east corner of that county, there enters the ocean.

Imagine the general plane of the chalk, as it sinks to the south-east, once divested of its diluvial covering. The line at which this plane would be intersected by the ancient ocean, defines an irregular area, which is precisely that occupied by the crag formation.

It is unnecessary to enlarge, in detail, upon the geological minutiæ of this district, but it is essential to our subject to consider its principal characters.

All admit that the chalk, more than any other formation, exhibits the powerful effects of immense currents, sweeping over its surface: that valleys have been hollowed out, and eminences formed, and a large portion of this island covered with its debris. From the variety of strata, some even of fresh-water origin, which occupy certain positions above the chalk, it is evident, that at distant intervals, considerable geological changes, more or less extensive, were effected. The imbedded vegetables, the zoophites, the shellfish, and the animals change with each deposit; the old series become extinct, and new ones in their turn become documents attesting geological epochs;—the unerring records of successive æras:—medals stamped, not with specific, but with relative dates.

The stratum of marine productions, under the local name of crag, has its assignable and comparative date; its inhabitants were the last that occupied the waters and the ancient shores, prior to the catastrophe which affected this part of our globe, and to the reforming from its wreck that surface on

* Borings were continued in the chalk at this place, 293 feet more.

† I adopt the term *diluvial*, now in general use, to indicate the water-worn debris resulting from the deluge; as distinguished from the *alluvial* deposits which proceed from causes yet in operation.

which

which man has fixed his abode and covered with his species. This crag rests in part upon the London clay, and a laminated clay without fossils, perhaps the plastic clay, and partly upon the chalk, occupying the lowest sites; rarely rising to 80 feet above the present level of the sea, and in general not more than half that elevation. The average level of its base may be considered to be about that of the present ocean. In certain cases, where the chalk hills attain a higher level than the crag, that deposit could only be expected to envelop or surround their sides, and not to penetrate *into the chalk*. Such eminences would then present the appearance of tongues or promontories of chalk, protruding into the crag; and this circumstance accounts for the occasionally apparent absence of that formation.

But the crag itself has, at the last of the geological epochs, been subjected to abrasion by the diluvial currents to which allusion has been made. Portions, probably from its western edges, have been swept away. Their fragments, mingled with those of the chalk and preceding formations, piled in enormous heaps, form the cliffs of Cromer and Trimingham 250 or 300 feet in thickness, upon the original crag, which rests, *in situ*, at their base. The proof of the disruption and transportation of more ancient strata, may be observed in the enormous detached masses of chalk, in these diluvial cliffs, at various elevations *above* the crag. Near the light-house hill at Cromer, one of these insulated patches is 150 feet high, and has a kiln upon it, in which lime of an excellent quality is burned. Further on, at Runton, is a large mass 80 feet thick: another rises to the height of 100 feet; and at Sherringham is another still higher. In all these cases, they rest *upon* the crag, proving alike the breaking up of the older strata and the continuity of the later. (See the Section No. IV.)

We have yet to consider one remarkable accompaniment of the upper marine formation, upon the Norfolk coast. This consists in that apparently continuous bed of vegetable substances, with which the crag is frequently in contact, at an irregular elevation; sometimes above and sometimes below the high-water line. This coincidence had been remarked in 1822 along about 25 miles of the coast; but it was more obvious after the unusually high tides, in February 1825, had carried away large portions of the cliffs, leaving the woody stratum exposed. At some points this bed consists of forest peat, containing fir cones and fragments of bones; in others, of woody clay; and elsewhere of large stools of trees, standing thickly together, the stems appearing to have been broken off about 18 inches from their base. They are evidently rooted in the

clay

clay or sandy bed in which they originally grew, and their stems, branches and leaves, lie around them, flattened by the pressure of from 30 to 300 feet of diluvial deposits. It is not possible to say how far inland this subterranean forest extends; but that it is not a mere external belt is obvious from the constant exposure and removal of new portions, at the base of the cliffs.

Doubtless this must be the southern extremity of that submarine forest, which has long engaged the notice of geologists, on the north-west part of Norfolk, whence it is traced across the Wash, and the fens of Cambridgeshire to Peterborough, and all along the Lincolnshire coast, as far as the Humber. There is no important variation in the general level of this woody tract. As relates to the Norfolk portion, it appears so closely in connection with the crag formation, as almost to form a part of it: the shells of the one being occasionally mixed with the vegetable matter of the other; and are further accompanied by bones of stags, elephants and oxen.

An obvious similarity exists between the deposits on the Norfolk coast, and those in the district between the Humber and Bridlington bay. The same diluvial accumulations; the same description of large bones of animals, of shelly fragments; of crumbly slipping cliffs; of subterranean forests at their base;—the same traces of plastic clay above the chalk, and rolled masses of primitive rocks mixed with the alluvium, attest the contemporaneous origin of the Holderness district with that more immediately under consideration. Dr. Alderson, in describing the geological characters of that district *, many years ago, was of opinion that the diluvial hills were heaped upon the submarine forest. Nothing has arisen to discourage that idea; but it derives confirmation from the parallel case which is presented by the cliffs of Norfolk.

On the first view of this extensive subterranean or submarine forest, one is inclined to inquire whether it be not contemporaneous with the freshwater formations observed elsewhere above the chalk? Hitherto no freshwater shells have been observed imbedded in this deposit on the Norfolk coast; but they have been seen at Harwich, and in the clay cliffs of Essex; and fluviatile shells abound in the forest peat of the fens of Lincolnshire.

Limiting our observation at present to these sites of ancient woods and beds of peat on the east coast of this county, we perceive that they are so variable in position, so undulatory, so often concealed by diluvium, so changeable in their

* Nicholson's Philosophical Journal, 4to vol. iii.

appearance through every modification of woody clay and gravel, peat and beds of forest trees, that it is often difficult to determine whether their real position be above or beneath the crag. Certainly, near Cromer, the trees are a few feet above the crag stratum, and are about the level of high water.

Perhaps the most probable conclusions, to be derived from a consideration of all these circumstances, are these:—

That after the formation of the chalk, the waters deposited the marine exuviæ, and gave existence, during the long period in which they occupied that portion of the former surface, to those remarkable accumulations of crag shells which we now witness.

That the trees and vegetables covered various parts of the surface of this new formation after it had become consolidated.

That in this state, these woodland tracts afforded shelter and support to certain animals, whose traces we find both amongst the vegetable deposits and in the drifted heaps containing marine substances.

Finally: All were buried in one common catastrophe. The same eruption of the waters that overthrew the pines and forest trees, destroyed the herbivorous animals, and buried the crag shells, beneath the ruins of more ancient strata.

[To be continued.]

LVIII. *Astronomical Observations* 1827. *By Lieut.* George Beaufoy, *R. N.*

Bushey Heath, near Stanmore.

LATITUDE 51° 37′ 44″·3 North. Longitude west in time 1′ 20″·93.

Observed transits of the moon, and moon-culminating stars over the middle of the transit instrument in sidereal time.

1827.	Stars.	Transits.
Feb. 13.	62 g Leonis	10° 54′ 47″·64
13.	69 Leonis	11 04 56 ·62
13.	Moon (18)	11 20 41 ·16

Eclipses of Jupiter's Satellites.

Feb. 17th. Emersion of Jupiter's 3d satellite.	12^h 45^m 41^s·06 M. T. at Bushey.	12 47 01 ·90 M. T. at Greenwich.
Feb. 22d. Immersion of Jupiter's 1st satellite.	14 28 23 ·77 M. T. at Bushey.	14 29 44 ·70 M. T. at Greenwich.
Feb. 24th. Immersion of Jupiter's 3d satellite.	14 00 25 ·07 M. T. at Bushey.	14 01 46 ·00 M. T. at Greenwich.
Feb. 24th. Emersion of Jupiter's 3d satellite.	16 42 21 ·08 M. T. at Bushey.	16 42 42 ·00 M. T. at Greenwich.

LIX. *Notices*

LIX. *Notices respecting New Books.*

British Entomology, or Illustrations and Descriptions of the Genera of Insects found in Great Britain and Ireland; containing coloured Figures from Nature of the most rare and beautiful Species, and of the Plants upon which they are found. By JOHN CURTIS, F.L.S.

SINCE this valuable work was last noticed in the Philosophical Magazine the third volume has been completed, in the same style of accurate and beautiful execution in its figures and dissections, and of authentic and full information in its scientific details, which had so justly recommended it to the students of Entomology.

In this volume 4 new genera have been established, and characters given of 19 others which had not appeared in any British work. Of the Plates, 22 are of species never before figured, and of the remaining 26, 7 only have been figured in this country. The author states "that the generic characters have all been described from actual observation, except where acknowledgements are attached; instead of being taken, as was at first proposed, from Latreille and other authors; and that the figures, both of the insects and plants, are all from the author's original drawings, with the exception of a few of the caterpillars, which have either been supplied by friends or copied from German works; and in addition to many local and rare plants, he has been so fortunate as to record a new British species, *Mespilus Cotoneaster.*—The original plan has also been somewhat enlarged by the synoptic view that is given of each genus, which when the work is completed will render it the most perfect that has ever appeared in this country; and the references that are given to all the species, will enable any one to study and obtain a perfect knowledge of the individuals comprised in each genus, thereby imperceptibly leading him to a knowledge of the whole system."

Three numbers of the fourth volume have also appeared, containing many interesting subjects.

LX. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY OF LONDON.

Feb. 9.—REPORT of the Council of the Society to the seventh Annual General Meeting this Day:—

SEVEN years have now elapsed since the formation of this Society: during which period, it must be evident to every intelligent observer, that a considerable progress (assisted, it is hoped, by the exertions of this Society) has been made in the science of Astronomy, not only in our own, but in every other country. The increased number of observatories, and the consequent encouragement which is given to the improvement of astronomical instruments:—the zeal and assiduity, not only of the public observers, but also of many private in-

 dividuals,

dividuals, who nobly sacrifice a great portion of their time and fortune to this laudable pursuit,—prove that the science is now more generally followed and encouraged than at any former period.

To enable the Society to continue their assistance in preserving and promoting this favourable change, the Council rely on the cordial cooperation of all those members who have the means, in their power, of conducing to this grand end. Those even, who have only *small* instruments in their possession, may still do much good, by a careful and judicious use of them: for although every astronomer must admire the vast field that has been opened by the powerful and splendid telescopes of Messrs. Herschel, South, and Struve, and the patience and skilful assiduity of these observers, yet we ought not to lose sight of those innumerable aids which may be rendered to astronomy, by more humble instruments, nor of the assistance that may be afforded to the *physical* and other departments of the science by those who are not possessed of any instrument at all.

In the last Report of the Council it was stated, that a letter had been received from M. Bessel, relative to a plan for a general survey of the Heavens, and for making detached Charts of the same. The *prospectus* relative to this subject was translated and distributed, not only amongst the members of this Society, but also amongst such other astronomers, as might be supposed desirous of encouraging so useful and important an undertaking. Two applications were made from this country, to the Committee at Berlin, appointed to superintend the distribution of the allotments; but, it is doubtful whether more than *one* of them can be appropriated here, as it is understood that the rest have been, or probably will be, taken up by different astronomers on the continent.

The Council regret that all the various prize questions, that have from time to time been proposed by the Society, still remain unanswered: the period having expired for the determination of the whole of them; except that which relates to the moon's place, which will not terminate till Feb. 1st in the ensuing year. How far it may be expedient to renew them, or to substitute others, will depend on the views entertained on this subject, by their successors in office.

The new Tables for computing the Aberration, Precession, and Nutation of 2881 principal fixed stars, together with a Catalogue of the same, are now completed, and have been some time in the hands of the public. This important work was first suggested, and the formulæ for the computations were investigated and practically arranged by F. Baily, Esq.

your

your indefatigable President, who, agreeably to the Regulations of the Society, resigns the chair this day. Much of the time and labour of the computers, engaged in this extensive work, was saved, and the liability to error very much abridged, by the use of printed *skeleton forms*, which he had constructed expressly for their use from formulæ reduced to the most simple and convenient shape for calculation. The work itself has been brought to a successful termination by the extraordinary diligence, activity and perseverance of Lieut. Stratford, of the Royal Navy, one of your Secretaries; who, in the midst of his other various avocations and duties, has been unremitting in his attention to promote the progress and secure the accuracy of this highly useful work; and who is entitled to your best and most cordial thanks for such a devotion of his time and labour. In fact, the Council, desirous of expressing their sense of the benefit conferred on the science of Astronomy by this important undertaking, have awarded the gold medal to Mr. Baily; and the silver medal to Lieut. Stratford, for the service rendered by these gentlemen in the promotion and completion of the work. May we hope that some experienced astronomer will now take up this new catalogue, and make a series of observations on *every* star contained therein, whereby we may be enabled to ascertain more correctly the proper motion (if any) that should be attributed to each star: and thus deduce a *Fundamental Catalogue* that may assist astronomers for many years to come. The expense of computing and printing this Catalogue has encroached on the ordinary funds of the Society; and has induced many members to suggest the propriety and advantage of defraying the expense not only of this, but of any similar undertaking, by means of a separate subscription amongst the members. Should a measure of this kind be recommended, the Council trust that it will meet with the support of every friend of Science*.

The Council, bearing in mind the objects which it is the wish and desire of the Society more particularly to promote, have also awarded the silver medal to Col. Beaufoy for his valuable collection of observations communicated from time to time to this Society, and more especially those relative to the Eclipses of Jupiter's Satellites. Part of this collection has already been published in the Memoirs of this Society; and the remainder will appear in the ensuing volume. These observations seem to have been made with great care and diligence, and afford

* [The subscription was immediately set on foot, and met with very considerable support. The list of subscribers is in the hands of the Secretary, and may be seen by any of the members.—*Sec.*]

another

another and a powerful instance how much the science of astronomy may be benefited by the active exertions of *one* individual. Phænomena of this kind cannot always be observed at the *public* observatories: the state of the weather, or more important avocations, may oftentimes interfere to prevent it. It is in such cases, and in numerous other instances, that *private* observers may render an important benefit to the science by their active cooperations.

These several medals to Mr. Baily, Lieut. Stratford, and Col. Beaufoy will be presented at a subsequent general meeting of the Society to be convened for that express purpose.

The past year has been abundant in the discovery of Comets; no less than five having been announced at the last meeting of the Society. One of these was discovered by M. Gambart, the celebrated astronomer at Marseilles; who, on computing its elements, found that, in all probability, it would pass over the sun's disc on the morning of the 18th of November. He immediately adopted measures for communicating the result of his calculations to all the astronomers in Europe, in order that they might witness this remarkable appearance. But, unfortunately, the whole of that day was cloudy; and it does not appear that this singular phænomenon was witnessed by any human being. To M. Gambart we are also indebted as one of the discoverers of another comet, which appeared in the month of March; and which has since been found to be *periodical*. This comet had been previously seen by M. Biela at Josephstadt: and M. Clausen, on computing its elements, ascertained that it was the same as that which was seen in 1772 and again in 1805. It is remarkable that both M. Biela and M. Gambart had, in the mean time, come to the same conclusion, from the elements deduced from their own separate and independent observations; thus confirming the addition of another revolving comet to our system, whose period is about 2461 days, or about twice the period of the celebrated comet of Encke. This new planetary body will make its appearance again about the latter end of the year 1832: and the attention of astronomers will then be naturally directed towards its return. If the comet of 1786 bear the name of Encke, this new revolving comet ought, for a similar reason, to bear the name of that astronomer who may most effectually succeed in investigating the laws by which it is governed. It is but a just tribute of respect to men who, by their assiduity and talent thus enlarge the bounds of science, and add to that vast mass of facts which are absolutely necessary to enable us to judge of the true system of the universe.

In the last part of the Memoirs of this Society, is a Report from

from the Committee appointed to examine the telescope, whose object-glass was formed of the glass presented to the Society by the late M. Guinand. The object-glass being finished and approved by the Committee (whose report will be seen in the last volume of the Memoirs) it was thought advisable that it should be offered for sale to any of the members of the Society that might be disposed to bid for it: and that the proceeds, after the payment of expenses for working the glass, should be transmitted to the family of M. Guinand, for their use and benefit. This has been done: and the object-glass is now in the possession of the Rev. Dr. Pearson, the Treasurer of this Society.

With respect to the finances of the Society, it will appear from the report of the Auditors, which has been read, that there have been elected 11 new Members, and 3 Associates since the last anniversary: and that the Society now consists of 212 Members and 32 Associates:—in all, 244. At the same time it will be seen that considerable expenses have been incurred in printing the last volume of the Memoirs: which, however, contains a considerable quantity of matter that must be interesting both to the theoretical and practical astronomer.

Amongst the losses by death, which the Society has sustained in the year just past, the Council have to regret those of three of its distinguished Associates: MM. Bode, Fraunhofer, and Piazzi. The first has been long known not only as the able conductor of the Ephemeris published annually at Berlin, (a work which for many years tended more than any other to promote the advancement of astronomy, by the circulation of important and useful information on various branches of the science,) but also as the author of several valuable werks conducive to the same end; amongst which his Catalogue of 17,240 stars (reduced from the observations of various astronomers), and his Charts of the same, may be considered as the most important. He died at the advanced age of 80 years.

M. Fraunhofer has long been celebrated as a distinguished optician, and as an artist of the first class. Few of the specimens however of his superior talent have reached *this* country: but on the continent, where they are more numerous, their value is highly appreciated. Though not an Astronomer himself, he has the strongest of claims to the respect and gratitude of Astronomers, in furnishing them with means of discovery, in which the most exquisite skill in point of practical execution was directed by the utmost refinement of theoretical knowledge. He was in the highest sense of the word, an optician, an original discoverer in the most abstruse and delicate departments of his science,

science, a competent mathematician, an admirable mechanist, and a man of a truly philosophical and scientific turn of mind.

Raised by his extraordinary talents from the lowest station in a manufacturing establishment, to the direction of the optical department of the business in which he originally laboured as an ordinary workman, he applied the whole power of his mind to the perfection of the refracting telescope. Easily mastering the refinements of its theory, he saw with regret that they were for the most part unavailable in practice for want of precise knowledge of the optical properties of the materials used. This he set about to remedy; and by a series of admirable experiments (of which it is impossible in a report of this nature to give any idea) succeeded in giving to optical determinations the precision of astronomical observations, surpassing in this respect all that had gone before him, except perhaps his great predecessor NEWTON. He had, it is true, advantages in these researches (such as neither Newton nor any other experimenter has ever possessed) in a command of apparatus limited only by his own inventive powers. It was in the course of these researches that he was led to the important discovery of the dark lines which occur in the solar spectrum. In this, indeed, he was in some degree anticipated by an illustrious countryman of our own, to whose powers universal science bears grateful testimony. But it is certain that he had no knowledge of the facts thus previously ascertained, and that he pushed his discovery to a point very far beyond them: being aided in so doing by possessing the happy secret of manufacturing flint glass of perfect homogeneity. Whether he originally invented this process, or procured it from another, this is not the occasion to pronounce; but at least his own distinct assertion that he brought it to its final state of perfection, and to a certainty of manipulation by his personal investigations, ought not to be doubted. Nor did he suffer the secret to lie idle or useless,—his telescopes are scattered over Europe; and his last splendid performance has already demonstrated, by the results it has afforded, his claims to unbounded admiration as an artist. The mechanism employed in the working of his glasses, his mode of centering and adjusting them, and every other part of his processes (the fabrica ion of his glass only excepted) has been witnessed by more than one member of this Society. It bore the stamp of all his works—simplicity, regularity, and incomparable neatness and precision.

Of his other valuable experiments and discoveries in physical optics, connected with the interferences of the rays of light, (in all of which, though pushed far in advance of the actual

state

state of knowledge, he appears to have relied entirely on his own resources, and drawn little from others,) as less connected with astronomy, it is not necessary here to speak. He died at a premature age: his death being accelerated, it is said, by the unwholesome nature of the processes employed in his glass-house; leaving behind him a reputation rarely if ever attained by one so young.

Of M. Piazzi and his labours, it will also be interesting to the Society to receive a concise account, as this distinguished individual furnishes another example, in addition to the many already upon record, of the power of genius to deliver a man from pursuits for which he had no taste, and to carry him successfully through others, to promote which he was richly qualified *. Piazzi was born at Ponte in the Valteline, July 16th 1746, and died at Naples, July 22d 1826. Early in life he was devoted to a religious order denominated the *Théatins*, at Milan. But, after various changes, he in 1780 accepted the appointment of Professor of the Higher Mathematics in the Academy of Palermo: and from that time, entirely devoted himself to science.

In a few years he obtained the confidence and favour of the Prince of Caramanico, viceroy of Sicily, by whose permission and assistance he founded an observatory at Palermo. With a view to open an intercourse with astronomers, and to obtain valuable instruments for his observatory, he visited England, where he formed an intimacy with Maskelyne, Herschel, Vince, and Ramsden. From the last of these he obtained some very excellent instruments, and, amongst the rest, the Altitude and Azimuth instrument, with which his principal observations were made. From this time Piazzi cherished a warm attachment both to the English and to their language; and to the latest period of his life continued to evince the same esteem. While he was in England, Piazzi observed at Greenwich, in conjunction with Maskelyne, the solar eclipse of June 3d 1788. He also collected the corresponding observations of eighteen different astronomers in various parts of Europe, and deduced from them the differences in longitude of the several observatories from that of Greenwich. The results he published in the *Philosophical Transactions* for 1789 (vol. lxxix); and the circumstance is here recorded, as this paper is understood to be M. Piazzi's earliest production as an astronomer. In 1789 he commenced with great activity, his labours in his new observatory, then the most southern which existed in Europe; that at Malta having been recently destroyed by fire.

* A Memoir of this distinguished Astronomer will be found in our last number.—Edit.

On the 1st of January 1801 he discovered the planet *Ceres.* The principal circumstances of that discovery, being well known to astronomers, need not be detailed here. But it is due to the character of this distinguished individual, to state, that when the king of Naples announced his intention of perpetuating the event by the circulation of a gold medal among European observers, Piazzi, whose modesty and zeal were equal to his merit, requested the monarch to assign the proposed value of the medals to the purchase of an *equatorial,* which he thought was greatly needed in his observatory. In 1803 he published the result of a labour of twelve years, undertaken with a view to determine *the mean position of the principal stars;* for this work he received the medal from the Royal Academy of Sciences at Paris. In 1814 was published M. Piazzi's *New Catalogue,* from which it appeared that this indefatigable astronomer had actually extended his researches to 7646 stars! Early in 1817 M. Piazzi published his *Lessons on Astronomy,* and the same year he was called to Naples, to put into activity the new observatory established on the heights of *Capo-di-Monte.* Cacciatore (now also one of the Associates of this Society) has from that time taken the charge of the observatory at Palermo; and by his zeal and assiduity is emulating the conduct of his predecessor.

The subsequent labours of this indefatigable astronomer, are as universally known as they are highly appreciated, throughout Europe. The grand work, however, to which we have already adverted (*the Catalogue of* 7646 *stars*) will ever remain a monument of his superior activity and perseverance, as long as the science endures. This important work far exceeds every thing of the kind that has preceded it; and shows more powerfully than words can express, what may be effected by the talents and assiduity of *one* individual.

The will of this eminent astronomer furnishes a new proof of his cordial desire to contribute perpetually to the promotion of his favourite science. He has bequeathed his library and all his instruments to the observatory at Palermo; and has assigned a liberal annuity to be devoted in succession to the instruction of young men who evince a marked partiality for this interesting department of knowledge.

The Council trust that the several members of this Society require no additional excitement to promote and advance the cause in which they have so laudably embarked. They should recollect, however, that without their cordial cooperation and assistance, the labours and efforts of the Council will be in vain. For, the *Council* are merely the officers of the Society, and can only collect and arrange the subjects that present themselves.

themselves. To the *individuals* of the Society it more properly belongs to furnish those subjects which may tend to the improvement of the science, either theoretically or practically. The Council have, indeed, in some of their former Reports, ventured to suggest several points as more particularly worthy of the attention of the Members, but it must be obvious to every one, that these are a *few* only of the desiderata in Astronomy; and that many others will suggest themselves to every skilful and intelligent observer.

The meeting then proceeded to the election of Officers for the ensuing year, when the following List was delivered in by the scrutineers: viz.

President: J. F. W. Herschel, Esq. M.A. F.R.S. L. & E. M.R.I.A. & F.G.S.—*Vice-Presidents:* Capt. F. Beaufort, R.N. F.R.S.; Lieut.-Gen. Sir T. M. Brisbane, K.C.B. F.R.S. L. & E.; Henry Thomas Colebrooke, Esq. F.R.S. L. & E. F.L.S. & G.S.; James South, Esq. F.R.S. & L.S.—*Treasurer:* Rev. William Pearson, LL.D. F.R.S.—*Secretaries:* Olinthus G. Gregory, LL.D. *Prof. Math. Roy. Mil. Acad. Woolwich;* Lieut. W. S. Stratford, R.N.—*Foreign Secretary:* Charles Babbage, Esq. M.A. F.R.S. L. & E. & M.R.I.A.—*Council:* Francis Baily, Esq. F.R.S. L.S. & G.S. & M.R.I.A.; Colonel Mark Beaufoy, F.R.S. & L. S.; Lieut.-Col. Thomas Colby, R.E. LL.D. & F.R.S. L. & E.; Capt. George Everest; Davies Gilbert, Esq. M.P. V.P.R.S. F.L.S. & G.S.; Benjamin Gompertz, Esq. F.R.S.; Stephen Groombridge, Esq. F.R.S.; James Horsburgh, Esq. F.R.S.; Rt. Hon. Lord Oxmantown; Edward Riddle, Esq.

March 9.—At this meeting there was read, a "Notice respecting some errors common to many tables of logarithms," by C. Babbage, Esq. Foreign Secretary of this Society. Mr. Babbage having lately printed a stereotype table of the logarithms of the natural numbers for the use of the Trigonometrical Survey in Ireland, for the sake of greater accuracy subjected them to eight readings and comparisons with other tables. This cautious process led to the detection of various errors, which are common to almost all the tables; those of Vega, the last impressions of Callet, and Mr. Babbage's own tables, being all that he has found free from the errors which he specifies. The tables subjected to this examination, were those of Vlacq, Gouda, 1628, carried to ten figures; Vlacq, London, 1633; Wingate, London, 1633; Newton, in his *Trig. Britan.* 1658, to eight figures; Sherwin, London, 1726; 2nd ed. 1741; 3rd ed. 1742; Gardiner, London, 1742; Sherwin, 4th ed. 1761; 5th ed. 1770; Gardiner, Avignon, 1770; Schulze, Berlin, 1778; Gardiner, Firenze, 1782; Taylor, London, 1792; Vega, Leipsic, 1794; Callet, (stereotype,) Paris,

 1795.

1795; Callet, ditto, Paris, (tirage,) 1825; Hobert and Ideler, Berlin, 1799; Delambre, Tab. Dec. Paris, 1801; Hutton, 4th ed. London, 1804, 5th ed. 1811; Vega, Leipsic, 1820; Hutton, 6th ed., London, 1822; Babbage, London, 1827.—Mr. Babbage thinks that the errors which he has detected can only be attributed to the universal system of copying which prevails in such works.

The numbers and *correct* logarithms to *seven* places are as below:

Numbers.	Logarithms.	Vlacq's last five figures.
24626	3913940	39751
38962	5906412	13420
57628	7606335	35875
57629	411	10436
63747	8044598	97412
67951	8321959	58424

From these the several tables specified may readily be corrected.

Mr. Babbage knowing that there was in the Library of the Royal Society a table of logarithms printed in the Chinese character, and which exhibits no indication or acknowledgement of its being copied from another work, was naturally desirous to compare it with European tables. On doing so, he found that in the *six* cases above noted, errors occurred precisely as in the European tables; thus furnishing an irresistible proof that the Chinese tables have an European origin.

There were next read two letters from Mr. Andrew Lang to F. Baily, Esq.: one dated St. Croix, 20th of March 1826; the other, St. Croix, 30th of November 1826. The first of these transmits an account of observations of the meridian transit of the moon's enlightened limb, and some stars preceding and following her, made at St. Croix, lat. 17° 44′ 32″ north, assumed long. 64° 45′ west, between September 22, 1825, and March 15, 1826. These were sent to Mr. Schumacher at the same time, and have been published in No. 104 of his *Astron. Nachrichten.*

Mr. Lang describes the climate of St. Croix as peculiarly favourable to astronomical observations, and speaks of the steadiness of the terrestrial refraction there. The terrestrial refraction scarcely ever varies perceptibly from the *one-sixteenth* part of the intercepted arc.

In Mr. Lang's second communication he presents a further account of the meridian transits of the moon's enlightened limb, and of moon-culminating stars, observed between March 30, and November 21, 1826. He also gives a summary of his observations of occultations of μ^1, and μ^2, *Sagittarii* by the moon, on the 9th of September; and of ψ *Virginis*, on the 28th of October.

Next, there was read a paper, "On a new application of the method of determining the time by observations of two stars when in the same vertical, to the case of *Polaris* when so situated with respect to any other circumpolar star in the course of its diurnal revolution below the pole: By Dr. T. L. Tiarks. The author first describes the peculiarities and advantages of this method, and then presents

presents the investigation of the formulæ of computation. If l denote the co-latitude of the place of observation, d the polar distance of the pole-star, D that of the other star, α their difference of right ascensions, and t the time elapsed from the upper passage of the pole-star to the moment of its being on the same vertical with the other; then the result of the investigation gives

$$(\mathrm{I}) \quad \ldots \quad \sin(t+\phi) = \frac{\sin\alpha}{\gamma\tan l}.$$

The values of γ and ϕ being determined by the following equations: viz.

$$(\mathrm{II}) \quad \ldots\ldots \quad \gamma = \frac{\sin(D-d)}{\sin d\sin D\cos\psi}.$$

$$(\mathrm{III}) \quad \ldots \quad \tan\psi = \frac{\sin\frac{1}{2}\alpha\ \sqrt{(\sin 2D\sin 2d)}}{\sin(D-d)}$$

$$(\mathrm{IV}) \quad \ldots \quad \sin\phi = \frac{\sin\alpha}{\gamma\tan d}.$$

The author occupies a portion of his paper in tracing the limits of error, and in pointing out in what cases the method is not strictly true.

Lastly: There was read a letter from M. Gambart to the President, dated Marseilles, 30th of December 1826. After adverting to what may be supposed his temerity in anticipating the transit of the comet seen in Boötes over the sun's disc, on the 18th of November, he presents the elements of the parabolic orbit of another comet, which are as below: viz.

Passage of the perihelion 1827.	34ᵈ·989 M.T. from midn'.		
Perihelion distance	0·455		
Longitude of perihelion	34°	0′	50″
Longitude of the node	191	44	33
Inclination	72	4	15
Motion retrograde.			

M. Gambart exhibits a comparison of the results of these elements, and of his observations on the 27th, 28th, and 29th of December. He then adds a few remarks, which need not be recorded, and congratulates himself and astronomers generally, upon the existence and success of the Astronomical Society of London. "What," he asks, "may not be expected from so liberal an association? Happy the country where the love of science alone causes so many men of enlightened minds to combine in such an object! Happy, also, those who dwell there!"

The President read part of a private letter from M. Littrow, Director of the Imperial Observatory at Vienna, stating that His Majesty the Emperor of Austria has liberally authorized the purchase, for that observatory, of a refractor, similar in all respects to that made by Fraunhofer for the Observatory at Dorpat, and which at the death of that excellent artist was left (so he understood the words "*des noch übrigen*") by him amongst his other instruments undisposed of.

ROYAL

ROYAL SOCIETY.

Feb. 15.—Sir R. R. Vyvan, Bart. M.P., and Cæsar Moreau, Esq., were respectively admitted Fellows of the Society; and the following papers were read:

South Polar Distances of Stars included within the tropic of Capricorn; observed in the months of May and June 1822; reduced to their mean places for January 1823: with other astronomical observations: by C. Rumker, Esq.

This paper consists of, 1. A Catalogue of the south polar distances of about 204 stars in the Southern Hemisphere, arranged in a table accompanied by columns containing their annual variations in S. P. D. and their elements of aberration and nutation:

2. A determination of the latitude of the observatory at Paramatta, as deduced from circumpolar altitudes of β *Argus*, observed with the repeating circle; and which determination differs about 15″ from that obtained from solstices and zodiacal stars:

3. Observations of the summer solstice of 1822, with the mural circle:

4. Observations of the moon:

5. Observations of the comet of 1824 in the Lion, with its elements; as also of another comet discovered by Sir Thos. Brisbane, in the Lion, in the year 1825; and of another, designated as the great comet of 1825:

6. Observations of the opposition of Mars.

7. Intervals between the transits of the moon and those of fixed stars culminating nearly in the same parallel in the year 1826.

8. Observations of an eclipse of the moon at Paramatta, May 21, 1826.

The author regards the accuracy of these observations as inferior to that obtained by Jupiter's satellites.

9. Observations of the Northern solstice of the sun with a repeating circle of Reichenbach, at Paramatta, in the year 1826. The obliquity resulting from these observations differs only 0″·4 from that stated in the Nautical Almanac.

Remarks on a correction of the solar tables required by Mr. South's observations; by G. B. Airey, Esq. F.R.S. and Lucasian Professor of Mathematics in the University of Cambridge.

The reading was begun of a paper On the mutual attraction of the particles of magnetic bodies, and on the law of variation of the magnetic forces generated by rotation; by S. H. Christie, Esq. M.A. F.R.S.

Feb. 22.—G. W. Taylor, Esq. M.P., was admitted a Fellow of the Society; and the reading of Mr. Christie's paper was concluded.

The results described by Mr. Christie in a former paper, when a copper disc was made to revolve under a magnetized needle, appearing to him not likely to lead to an accurate knowledge of the law of magnetic attraction developed during rotation, from the effect of lateral attraction, he was induced to resume the inquiry, substituting a ring for the disc, expecting that as no lateral force would here be called into action, the results would be more uniform; and in this expectation he was not disappointed. One of the first phænomena

mena that he encountered, was a very great diminution of magnetic force, when a ring of the same weight was substituted for a disc; and pursuing this point of inquiry, he found, that in all cases of solution of continuity, not only by cuts in the direction of radii from the centre, but also in concentric annuli or otherwise, there is always a great loss of force; the magnetism of the whole being always much greater than the sums of that of the parts.

In reasoning on the experiments detailed, Mr. Christie concludes that the greater development of magnetism in a disc subjected to the action of revolving magnets, takes place, when the axes of the magnets are vertically under points bisecting the radii, and that the magnetism decreases very rapidly as they approach the edge; thus indicating that for a full development of magnetism, a continuity of substance in all directions from the point acted on is principally requisite. Various phænomena lead also to the conclusion that the reduction of the disc by concentric and radiating cuts into very small portions, would render its magnetism quite insensible.

The author next proceeds to investigate by experiments of the same kind, the law of variation of the magnetic force, regarded as depending on the distance of the revolving magnets from the suspended body. Assuming in this investigation, as a consequence of the principles proposed by other writers, that the action may be referred to a single point or pole in the copper ring, somewhat in arrear of the point vertically over the magnet, and also that the mutual action of this pole, and the single point near the extremity of each magnet to which its action may also be referred, is inversely as the 4th power of their distance, he found these laws to be established by the experiments, made in various ways.

Lastly, Mr. Christie enters into an analytical examination, the object of which is to ascertain how far the principle of time being required for the development of magnetism, will account for the phænomena; and the conclusion at which he arrives, is, that it will do so satisfactorily. In the course of this examination, he infers that in certain cases a retrograde rotation in the suspended disc might take place, and suggests the great confirmation which such a fact, if observed, would afford this theory.

A notice was read, entitled, "Correction of an error in a paper published in the Philosophical Transactions, entitled 'On the parallax of the fixed stars;' by J. F. W. Herschel, Esq., M.A. Sec. R.S."; and a paper was also read, entitled, "On attractions apparently magnetic exhibited during chemical combination; by W. L. Henwood, Esq.": communicated by Davies Gilbert, Esq. M.P. V.P.R.S.

March 1.—Dr. J. C. Prichard was admitted a Fellow of the Society; and a paper was read, entitled "On the structure and use of the submaxillary odoriferous gland of the Crocodile; by Thomas Bell, Esq F.L S.": communicated by Sir E. Home, Bart., V.P.R.S.

Beneath the lower jaw of the Alligator and the Crocodile, on each side, is situated a gland which secretes an unctuous substance of a strong musky odour. About two years since, the author of this paper discovered in it a structure which is without parallel in the glandular system of other animals. His observations were made on the common American Alligator. In this animal the external orifice

fice of the gland is situated about two-thirds of the length of the lower jaw backwards from the symphysis, being a longitudinal slit a little within the lower edge of the basis of the jaw, through which exudes the substance just mentioned. During warm weather, when the animal feeds freely, the secretion is copious; but in winter it is much diminished in quantity and is less powerful in scent. The gland itself is a simple follicle of an elongated pyriform figure, lying between the skin and the under surface of the tongue. In an alligator of four feet in length, it is about half an inch long and one-sixth of an inch in diameter. This gland is enveloped by extremely fine and delicate muscular fibres, disposed obliquely, consisting of two fasciculi passing repeatedly over and under the gland, which unite at its base into a long and slender round muscle, closely attached to the corner of the os hyoides, and following the course of another muscle apparently identical with the mylo-hyoideus in the mammiferous animals. The use of the muscle appears to be to bring the gland into a proper position for its discharge, and then to operate the discharge, by pressure.

The author, considering the situation of the gland near the mouth of the alligator, and the predatory habits of the animal, together with its voracity of fish, and the well-known partiality of fish for odoriferous oils and extracts, conceives, that this secretion acts as a bait, attracting the fish to such a position as will enable the alligator readily to seize them, in his usual way of seizing his prey, by snapping sideways at them.

The reading was also commenced, of a paper "Note on the chemical composition of two liquids lately proposed as powerful disinfectants, and on the action of those liquids on putrid animal matter." By A. B. Granville, M.D. F.R.S.

March 8.—MM. Morichini, Ehrman, and Ampère, were respectively *elected* Foreign members of the Society.

A letter was read by the Vice-President in the Chair, which had been received at the Foreign office, from M. Rumker, announcing his discovery of a comet in the southern hemisphere, in September last, at Paramatta.

The reading of Dr. Granville's paper was then concluded.—Mons. Labarraque, a pharmacien residing in Paris, proposed, two or three years ago, to employ chlorine in a liquid form, in lieu of Morveau's method hitherto adopted, for disinfecting air in which putrid animal effluvia are disseminated, and for arresting putrefaction in dead bodies. For this purpose he selected; 1st, a solution of the salt formerly termed oxymuriate of lime in water; and 2dly, a solution of carbonate of soda saturated with chlorine gas.

To these liquids Mons. Labarraque gave the names of *Chlorure d'oxide de calcium, et d'oxide de sodium,*" corresponding to our chlorides of lime and soda; and under those denominations he promulgated, with a becoming spirit of liberality, his discovery of their disinfecting properties. The promulgation, however, was not as one had a right to expect, accompanied by any scientific inquiry into the real constitution of the liquids, nor by any analysis of their ingredients. Nor was it followed by any attempt to explain the curious and important facts which they had brought to light, and which

which were in a short time confirmed by the observations of many. Labarraque's views were formed, in Dr. G.'s opinion, on mere assumption, and were adopted by the French and by all the English translators and commentators who assisted in making his discovery more generally known. No one seemed to doubt the correctness of those views, until Dr. Granville, who had occasion to use very extensively one of the liquids in question (that containing soda), undertook the analysis of that liquid, in order to ascertain its real composition, and he entered upon an inquiry into the phænomena resulting from the action of chlorine on animal matter in a putrid state. The results of these inquiries Dr. Granville has detailed at full length, in his paper in which a series of experiments is minutely described, from which the author conceives he has proved, that the supposed "chloride of *oxide* of sodium" in solution is in reality a mixture of

73·53 dry chloride of sodium
26·47 neutral chlorate of soda

100·00

with an excess of chlorine equal to twice the bulk of the water employed in preparing the liquid agreeably to Labarraque's own formula.

Besides detailing the several analytical and synthetical experiments in support of the above conclusions, Dr. Granville attempts to prove the accuracy of those conclusions by the application of the atomic doctrine, as well as by a calculation of the weight and measure of the chlorine gas required to form the disinfecting liquid; whence it appears that during the process of preparing that liquid five atoms of oxygen combine with one atom of chlorine to form chloric acid, which unites with one atom of soda to form the chlorate of soda; and five atoms of sodium combine with five atoms of chlorine to form five integrant atoms of chloride of sodium. These combinations are not due to the decomposition of water but to a peculiar arrangement of the elements contained in the solution. With regard to the quantity of chlorine gas employed, it appears to be very considerable. Twenty fluid ounces of the liquid contain 503·36 cubic inches of that gas (besides the free chlorine), which weigh, according to Thomson's tables, 583·815 grains; and as the soda dissolved in those twenty ounces of liquid weighs 311·185 grains; according to the atomic theory, the weight of the solid contents of that quantity of liquid ought to amount to 725 grains; which is precisely what Dr. Granville found on evaporating the whole of the liquid to dryness. If this be the real composition of Labarraque's liquid, it is clear that no such compound as the chloride of oxide of sodium exists in it, and its present denomination must be incorrect; Dr. Granville therefore recommends that it should be abolished, and that the simple name of Disinfecting liquid of soda should be substituted for it.

In another part of his paper the author endeavours to show that the singular properties of the above liquid are due entirely to the chlorine, and

and in no way to the agency of the salts contained in it. The same results are obtained when a simple solution of chlorine water is employed; but in that case the escape of the gas is considerable, and consequently offensive to the operator and assistants. This is not the case when the same quantity of chlorine is thrown into a solution of the two salts mentioned in the course of the paper; so that Dr. Granville infers that the presence of those salts serves to lessen the tendency of the free chlorine to escape in a gaseous form. When the two salts, alone obtained by evaporation, are redissolved in distilled water, no disinfecting effects are obtained, although so large a proportion of the chlorine enters into their composition; but if two measures of that gas, equal to twice the bulk of the solution, be thrown in, all the disinfecting properties are restored to the liquid.

Dr. Granville promises to lay before the Royal Society a continuation of his inquiries on this subject, in which he will describe the mode of action of the disinfecting liquid on putrid animal matter,—detail the new compounds that result from that action,—point out a method of ascertaining the presence of animal effluvia in the air by means of chlorine, applicable in time of infectious diseases; and lastly, suggest a more easy and œconomical process of preparing the Disinfecting liquid.

A paper was also read, entitled "On the permeability of transparent screens of extreme tenuity by radiant heat; by W. Ritchie, A.M." communicated by Mr. Herschel.

Mr. Ritchie states that invisible radiant heat from sources at elevated temperatures freely permeates thin transparent screens in the same manner as light; but as this doctrine, established by Professor Prévost and M. de la Roche, has been controverted, he thinks it necessary to demonstrate it by fresh experiments. To this end he covered a small aperture with a film of glass almost iridescent, and keeping it constantly cold, by blowing on it, below the temperature of the ambient air, he found that an air-thermometer on one side of it was not affected by a heated iron ball on the other, if the temperature of the ball was low; but that as the temperature was raised, though not to the point of visible ignition, the effect on the thermometer became sensible and even considerable. Several other experiments are adduced, confirmatory of the same doctrine; and the author finds that little difference of effect is observed, whether the screens be near to or far from the heated ball, *cæteris paribus:*—and this he considers as demonstrating that the effect was not due to secondary radiation from the screen.

March 15.—Capt. G. Everest, the conductor of the Trigonometrical Survey of India, was admitted a Fellow of the Society; and MM. Struve, Stromeyer, Plana, and Sœmmering, were respectively *elected* Foreign members.

A paper was read, entitled "Correction of an error in the reduction of the observations for atmospherical refraction at Port Bowen; by Lieut. H. Foster, R.N. F.R.S."

The reading was also commenced of a paper on Experiments for determining the mean density of the earth, made, with two invariable pendulums,

pendulums, at the mine of Dolcoath in Cornwall, by Mr. Whewell, M.A. F.R.S., and G. B. Airey, M.A. F R.S., Lucasian Professor of Mathematics in the University of Cambridge.

March 22.—The reading of the above paper was concluded, and an Appendix to it by Professor Airey, was read. We intend giving some account of these two communications in our next Number.

LINNÆAN SOCIETY.

March 6.—Read a paper by Thos. Bell, Esq. F.L.S. On two new genera of land tortoises.—These genera possess a peculiar interest as exhibiting the affinities by which the freshwater tortoises are connected with those inhabiting the land. Mr. Bell has named them *Pyxis*, and *Kinyxis*; and both are distinguished by a moveable joint, one in the sternum, and the other in the hinder part of the back, by means of which the shell can be completely closed. The species described are *Pyxis arachnoides,* a perfect land tortoise, with the anterior lobe of the sternum moveable, and capable of as accurately closing the shell as in any species of the freshwater box tortoises: *Kinyxis castanea:* and *Kinyxis Homiana,* a species forming a passage from the group of *Testudinidæ* to that of the *Emydidæ.*

March 20. Amongst the presents announced, was a collection of birds from New Holland sent by Alex. MacLeay, Esq., Secretary of the Colony, and formerly the much-respected Secretary of the Society.—A further portion was read of Mr. W. S. MacLeay's paper on the Birds of Cuba, in the introductory part of which the principles of arrangement adopted by Aristotle in the Animal Kingdom are investigated.

HORTICULTURAL SOCIETY.

Feb. 6.—The following papers were read: Upon destroying the mildew on peach trees; by Mr. John Mearns, F.H.S.—Upon the best mode of obtaining late crops of melons; by Mr. William Greenshields, F.H.S.—Upon pruning and managing standard apple and pear trees; by the same.—On the progress of Horticulture in the north of Europe, particularly in and around Riga; by Mr. F. H. Zigra.—On the cultivation of the Heliotrope and other tender plants in open borders; by Mr. John Mearns, F.H.S.—On pruning plum trees when trained to walls; by the same.—An account of the mode of managing peach trees in an early peach-house; by Mr. Walter Henderson, C.M.H.S.—On the phænomena of the rose of Jericho; by Mr. John Murray, F.H.S.—Some remarkably cheap woollen netting for protecting fruit trees, which had been manufactured in Wales, was laid upon the table.—Various fruits of the season, and flowers of several kinds of Camellias, were exhibited by different Fellows; and a variety of articles were sent from the Society's Garden for inspection.

Feb. 20.—The following papers were read: An account of some remarkable holly hedges and trees in Scotland; by Joseph Sabine, Esq., F.R.S. (Secretary).—On the culture of the pine-apple; by Mr. James Dall, gardener to the earl of Hardwicke.—On forcing asparagus;

ragus; by the same. Both these papers were communicated by the Cambridge Horticultural Society, as deserving the annual silver medal given by the London Horticultural Society to provincial Horticultural Societies in communication with it.—On the cultivation of Camellias in the open air; by Mr. Joseph Harrison.—An account of a plan for preserving grapes in vineries from insects; by Mr. Charles Harrison, F.H.S.—Observations upon metallic hot-houses; by Mr. W. M'Murrie, F.H S.—Upon the culture of the *Prunus Pseudo cerasus* or Chinese cherry; by Thomas Andrew Knight, Esq, F.R.S (President).—A journal of meteorological observations made in the Garden of the Horticultural Society at Chiswick during the year 1826; by Mr. W. B. Booth, A.L.S.—A fine collection of fruit of the best American apples, which had been sent to the Society by Mr. Jesse Buel of Albany in the state of New York, was exhibited.—A plant in flower of a single Waratah Camellia, raised from seed in the garden of the Comte de Vandes, at Bayswater, was also placed upon the table.

ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 2—An account was given by Mr. Alcock in the Lecture-room, of the applications lately made in France of the chloride of lime, and the solution prepared by passing chlorine through solution of carbonate of soda, as disinfecting agents. It appears that notwithstanding the affinity by which the chlorine is held in these bodies, that it is ready to act upon, and destroy any putrid miasmata that may be floating in the atmosphere in which they are exposed. If a cloth be dipped in a solution of chloride of lime, and exposed to a foul air, or placed over putrescent matter, as a disinterred corpse, that the noxious effluvia are quickly destroyed, and all injury to the persons present prevented, without any unpleasant effects from the presence of the chloride. The applications of this fact to numerous cases of infected atmosphere were pointed out, and also to the amelioration and cure of ulcers or putrescent sores.

The Library tables were as before covered with numerous objects of interest: amongst which was a specimen of deadly vegetable poison from the kingdom of Assam, with which the Assamese tip their arrows and spears. It has not yet been examined.

Feb. 19.—A communication on the principle of security in locks was given from the table by Mr. Ainger, from which it appeared that the various methods invented of conferring security on locks, might be considered as of two kinds; those which placed numerous obstacles to the passage of the key (usually called wards), and those which placed impediments to the motion of the bolt. The latter appeared to be the only methods which afforded security, and a lock of this kind constructed in Egypt was produced, which from historical records appears to have been known and used there for 4000 years. Its action was illustrated by large models, as was also that of Bramah's and other locks; and the perfect security, which could now be obtained by locks, was explained by a reference to the principles upon which they were picked, and the manner in which these were rendered deceptive or unavailing by the lock-maker.

A specimen

A specimen of a fungus gathered from the beech-tree was laid upon the Library table. The whole of its upper surface was covered by an exudation of fine resin, forming an uniform coat over it. Books presented to the Institution, and works in the press, were also laid upon the table.

Feb 16.—Mr. Brande gave an account from the Lecture table of the method of manufacturing dies for coining, including an account of the mode and of the circumstances connected with the striking of coin and medals. The nature of the steel required for the die was first considered, and that stated to be the best, as far as the experience of the speaker went, which was least acted upon by dilute sulphuric acid: and the manner in which it was forged, softened, and prepared for the artist, was then described; the progress of his work illustrated by numerous specimens, and the way in which the first piece of art, or the matrix, was made to produce punches, and these again dies to be used for striking the pieces of metal or blanks, fully explained. The hardening of the die, the guarding it by a ring of iron, the work it could perform in the coining-presses, the destruction of dies at the Mint, the preparation of the blanks, and the difference between striking coin and medals,—were fully described; as was also that curious operation of lettering the edges of the pieces. Many fine specimens of Mr. Wyon's workmanship were on the table.

In the Library was exhibited a very fine specimen of that rare bird *Meleagris ocellata* or *Dindon Œillé*. It was brought alive to this country in 1814, from the Bay of Honduras; but died soon after its arrival, and has been very finely preserved.

A very large skull of a Walrus was also upon the table, with numerous presents of books, rare works, and the publications of the week.

LONDON MECHANICS' INSTITUTION.

A quarterly general meeting of the members of this Institution was held on the 7th of March, to receive the Committee's Report of the proceedings of their quarter terminating on that day; from which it appeared that courses of lectures had been delivered by Professor Millington, On the application of mathematical science to mechanical subjects; by Mr. Cooper, On metallurgic chemistry, which had occupied the Wednesday evenings: and that the Friday evenings had been occupied by a variety of lectures on miscellaneous interesting subjects.

It appears that the purposes for which this Institution was established, are now in full operation, and carried on with much activity and advantage to its members; for, besides the lectures twice a week, they have a Reading Room, and a Circulating Library, containing nearly 3000 volumes, and classes for instruction in arithmetic, mathematics, drawing, the English and French languages, geography and writing; and also weekly meetings for mutual instruction in mechanical philosophy and chemistry.

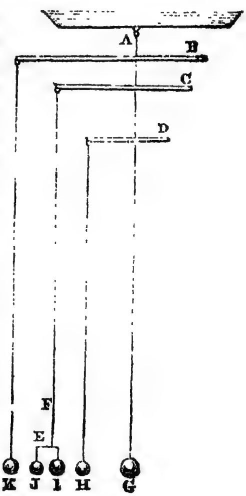
It was announced at the close of the meeting that Professor Millington

Millington was about to deliver a course of lectures on pneumatics, and that these would be followed about the middle of April, by a course of lectures on the structure and functions of the human body, by Dr. Birkbeck, the President.

LXI. *Intelligence and Miscellaneous Articles.*

DESCRIPTION OF A PLANETARIUM, OR ORRERY, ON A NEW PRINCIPLE, PUT IN MOTION BY THE STATE OF THE ATMOSPHERE.

IN the year 1824, I made a Planetarium or Orrery, on a new principle, showing the motion of the Earth and Moon round the Sun, and of the Moon round the Earth; of which invention I observed a notice in the Philosophical Magazine for March that year. I have lately constructed one on the same principle, representing the Sun, an inferior planet (Venus), the Earth and Moon, and a superior planet (Mars),—which I shall describe, referring to the figure which accompanies this paper. The figure is not given as an exact representation as to dimensions, but will, I imagine, with the description be found sufficient for those persons who may wish to make a planetarium of the same sort. The principal parts of this machine are the following; to which may be added rings and hooks for connecting the parts together.



A piece of catgut string about fifteen inches in length, A (represented as suspended from a beam in a room), this passes through the round bars or rods B, C, D, and is fastened to them on the under sides by knots with glue, to prevent them from slipping. E, a small bar or rod; F, a string of catgut about three feet long. G, H, I, J, K, five wooden balls to represent the Sun (which is gilt), Venus, the Earth, the Moon, and Mars. These balls are some suspended by sewing (linen) thread, and some by silk. They might all have been of either of those substances.

The Sun is hung below the catgut string A. Venus, from one end of the bar D. The Earth and Moon from E, which is suspended from the bar C (by catgut), and Mars from the bar B.

In order to balance the three large rods, I make use of flexible copper wire; and find a piece wound round spirally, very convenient for the purpose.

The

The annual motion of the planets round the Sun is caused by the property which catgut strings have of twisting and untwisting according to the state of the atmosphere. When the apparatus is removed from a damp room into one which is drier, and hung up, the catgut A twists and carries the rods B, C, D round from right to left, thus showing the course in which the planets really move round the Sun.

The small bar E hanging by a string of catgut about three feet long, makes *several* revolutions, whilst it is carried round the Sun *once;* the Earth and Moon are connected with this bar at unequal distances from the point of suspension, and are carried round this point (which represents the common centre of gravity between those bodies), the Moon by revolving round the Earth illustrating the monthly motion. Owing to the relative lengths of catgut between the point of suspension of the string A, and the bars B, C, and D, the difference in the annual motions of the planets is occasioned. In the Planetarium here figured, the diameter of the Earth's orbit is about one foot eight inches; that of the Moon's round the centre of gravity, between four and five inches. I have one, of the Sun, Earth, and Moon, in which the diameter of the orbit of the Earth is about six feet, and of the Moon about one foot eight inches.

On the principle of this machine, the rest of the Solar system usual on Orreries may be added; and where a considerable length of catgut can be conveniently made use of, the diurnal motions of the planets may be shown. By substituting a catgut string instead of the thread which suspends the Sun, its motion round its axis may easily be shown. By using a lamp instead of the gilt ball, or placing a candle at a proper height, eclipses of the Sun and Moon, and phases of the Moon, may be explained in a pleasing manner. It is suggested, in order to make the Earth keep its parallelism whilst revolving round the Sun, that a small terrestrial globe with an axis rendered magnetic be made use of; and if still greater accuracy be desired, that a bar magnet be placed according to the present declination of the Magnetic needle within a globe; but such a method does not appear to be necessary.

I am persuaded that a very amusing and instructive Planetarium on the plan of this may be made, and hope to see one with improvements. If a method can be adopted by which the planets can be placed at one point, to set out from, in their revolutions, it would be very satisfactory. Perhaps by having different pieces of catgut (instead of one string) to be connected by screws to the rods, this could be accomplished; but I think there will be some difficulty attending the attempt. This Apparatus I propose calling an "Atmospherical Planetarium, or Orrery," as it is put in motion by the state of the atmosphere as respects moisture, in the same way which catgut Hygrometers are. Perhaps some preferable substance to catgut may be met with; but I know not of any such. To prevent the strings entangling, the apparatus had better be removed in different portions than when connected together. I think it possible that a very eccentric orbit of a Comet may be contrived on the same principle

ciple as this Planetarium, but I have not yet attempted to make a ball revolve in such a course; to effect this object a much more complicated apparatus appears necessary than for a circular motion.—B. M. FORSTER.

Walthamstow, Essex, March 12, 1827.

CRYSTALLIZED LITHARGE.

M. Gaultier de Claubry remarked that crystals of litharge were formed during the cupellation of argentiferous lead: he collected and analysed some of them. These crystals had the appearance of regular dodecahedrons; but M. Beudant, who examined them with the assistance of the reflective goniometer, found that they possessed no regular angles, and that their facets were curvilinear.

M. Houton-Labilliadère had previously obtained litharge in crystals, which appeared to him to be regular dodecahedrons; they were formed in a solution of oxide of lead in soda, during the winter.—(*Ann. de Chim. et de Phys.* t. vii. p. 218.)

The crystals obtained during cupellation consisted of:

Protoxide of lead, with a trace of copper	96·3
Carbonic acid	3·3
	99·6

On the surface of the crystals there were semitransparent laminæ of a yellow colour, of the size of the nail; their composition was similar to that of the crystals.—(*Ann. de Chim. et de Phys.* t. xxiii. p. 443.)

COMPOSITION OF NITRIC ACID.

The 12th volume of the Annals of Philosophy, O. S. (p. 351), contains a translation of a paper on the composition of nitric acid, by Berthollet: the process employed was that of decomposing nitrate of potash by heat in a porcelain retort, the weight and nature of the gaseous products and of the residual potash being ascertained. From these experiments the author concluded that nitric acid is composed of 69·6 oxygen + 30·4 azote, instead of 74·08 of the former and 25·92 of the latter element, as now generally admitted.

Dr. Thomson observes, that though he has no doubt of the inaccuracy of Berthollet's analysis, he cannot pretend to account for the fallacy. Having lately prepared some oxygen gas by decomposing nitre, I found that the last gaseous product, if not entirely azotic gas, contained so little oxygen that it extinguished a candle. Upon pouring water into the gun-barrel to remove the potash, I found that oxygen gas was immediately evolved, and in such quantity that an ignited stick was immediately inflamed; and the combustion continued for a considerable period.

Now Berthollet distinctly, though erroneously, asserts, that the potash retains no oxygen: but it is evident from the experiment now stated,

stated, that peroxide of potassium is formed; and it appeared to me probable, that the quantity was sufficient to supply the deficiency of about $4\frac{1}{2}$ per cent of oxygen in Berthollet's experiment.—R. P.

PHOSPHURETTED HYDROGEN GAS.

M. Viala finds that when phosphorus is introduced into a receiver containing a weak solution of an alkali, phosphuretted hydrogen is formed, and evolved in a few hours without the application of heat.—*Journ. de Pharm.* Feb. 1827.

ACIDS DISCOVERED IN CASTOR OIL.

MM. Bussy and Lecanu have obtained three new fatty acids from castor oil: one, which they call *ricinic acid*, is fusible at 72° Fahr.; another, termed *elaiodic acid*, is fluid at several degrees below 32°; and the third they have denominated *margaritic acid;* this crystallizes in fine scales, and is not fusible below 264°. These acids are volatile, more or less soluble in alcohol, and perfectly insoluble in water; and they form salts of very distinct characters, with several bases, and especially with magnesia and oxide of lead.

When castor oil is distilled in a retort in the common way, there are obtained a small quantity of gas, water, and acetic acid, a colourless crystallizable volatile oil, ricinic and elaiodic acids, which condense with the oil in the receiver, and a solid matter which remains in the retort. The quantities of acid and of the volatile oil are nearly equal, and form nearly a third of the oil employed; the solid matter constitutes nearly the remaining two-thirds.

This is a very singular substance: it is of a yellowish white colour, full of cavities, and somewhat resembling the crumb of new bread. It is insoluble in water, alcohol, æther, the volatile and fixed oils. It is dissolved by the alkalies, with which it forms a kind of soap. It is not decomposed at a high temperature, inflames when exposed to an ignited body and burns very readily without melting. When, instead of distilling castor oil, it is treated with a solution of potash or soda, it saponifies even more readily than olive oil, and there are formed ricinates, elaiodates, margaritates and glycerin. No other product appears; the glycerin amounts to about a fifteenth part of the oil, the margaritic acid about one-thousandth, and the remainder is constituted of the other acids. These salts are very soluble in water, and act like ordinary soaps; the smallness of the quantity of margaritic acid will account for its not being found in the product of the distillation.—*Ibid.*

SUPPOSED CHLORATE OF MANGANESE IN THE NATIVE PEROXIDE.

Mr. Mac Mullin having observed (Institution Journal, vol. xxii. p. 231) when sulphuric acid is added to peroxide of manganese that chlorine is evolved, he conceived it might be derived from an admixture of muriate of manganese, iron, or copper; but having washed some of the peroxide with water, he did not find that any chloride of silver was precipitable from it; he therefore concluded that the peroxide in question contained no muriatic salt.

Mr. Mac Mullin continued his experiments to discover the source of the chlorine, and concludes "that the chlorine combined with the black oxide is in the state of chloric acid; and that the native oxide is, at least in part, and probably in proportions varying with the different specimens of the ore, a native chlorate of manganese."

I had prepared some observations, and at considerable length, to prove that the author of the above paper has been completely misled by forced analogies and erroneous experiments; but it afterwards occurred to me, that it would be better to show in a few words, the real source of the chlorine in question, the evolution of which from peroxide of manganese I had noticed, some time previous to the publication of Mr. Mac Mullin's paper.

I procured first some common peroxide of manganese, a second and pure specimen from Warwickshire, and a third crystallized variety from Germany: these were reduced to powder, and on the addition of sulphuric acid, chlorine was evolved from each. I then washed separate portions of them with distilled water; and on the addition of nitrate of silver to the washings, chloride of silver was immediately precipitated: sulphuric acid being poured upon the washed peroxide, no chlorine whatever was evolved; but being unwilling to trust merely to my own observation, I added sulphuric acid to an unwashed portion and to one which had been washed;—a by-stander immediately detected the odour of chlorine in the former, but not in the latter case.

To determine the nature of the salt from which the chlorine was evolved, I evaporated a portion of the washings very low, in order that, if any common salt were present, it might crystallize. I was, however, unable to procure any of it; sulphuretted hydrogen indicated no appearance of any metallic muriate, but oxalate of ammonia showed that lime was present, and nitrate of barytes gave sulphate. I conclude therefore that the native peroxide of manganese usually contains a small admixture of muriate and sulphate of lime. —R. P.

ARRIVAL OF MAJOR LAING AT TIMBUCTOO.

We are happy to learn that letters have been received from Major Laing, dated subsequent to his arrival at Timbuctoo; but by some oversight, the particular date is not inserted. The state of this city, so much talked of, and so much sought after by Europeans, together with the rivers and the country adjoining, will soon be made known, and by a hand fully able for the work. We regret, however, by these letters to learn that, instead of proceeding down the river Niger, as he intended, Majo. Laing intends returning home by way of Tripoli. What has occasioned this change in his route, whether ill-health, or finding insurmountable obstacles to his progress eastward and southward, we have not heard, and cannot take upon ourselves to determine.—*Glasgow Courier.*

HYBERNATION OF THE BLACK ANT.

On the 18th of January a large elm-tree, to all appearance sound, was cut down on the estate of Mr. Baden Powell of Lackington Green,

Green, near Tunbridge Wells. On examining the lower part of the trunk close to the root, a large excavation was discovered, rendering the base of the tree quite hollow: this cavity was filled with a large nest somewhat resembling a wasp's nest, but of looser materials, being composed of cells or separate excavations, the sides of which were tough and pliable, and of a brownish colour, smelling strongly of the sap of the tree, and filled with innumerable large black ants and their eggs, quite alive; that is, not torpid. The tree had evidently been excavated by them, and would in all probability have, ere long, failed in its accustomed foliage, the cavity being very large; it appeared indeed to measure above a foot in height, and the same in diameter, tapering towards the upper part. I am not aware that the nidus of this species of ant has ever been described; and should any of your correspondents wish it, I have not any doubt but a drawing might be obtained, as the nest is preserved.—T. Forster.

OBSERVATIONS ON A COMET; MADE AT PARAMATTA IN SEPTEMBER 1826. BY C. L. RUMKER.

1826.	Sidereal time.	Right Ascension.	Declination S.
Sept. 4	2^h 55^m 0^s	84° 8′ 27″	
	4 19 19	84 13 47	8° 49′ 0″
5	1 17 10	85 43 33	
	1 19 0		7 58 8
6	2 23 9	87 27 35	6 53 43
7	3 44 20	89 13 47	5 49 8
8	2 4 25	90 48 44	4 49 15
9	3 19 0	92 37 20	3 39 7

SCIENTIFIC BOOKS.

Just published.

Outlines of Human Physiology, by Herbert Mayo, surgeon, and lecturer on anatomy.

An Introductory Lecture on Human and Comparative Physiology, delivered at the New Medical School in Aldersgate street. By Peter M. Roget, M.D. F.R.S. &c.

Rheumatism, and some Diseases of the Heart and other Internal Organs; considered in the Gulstonian Lectures, read at the Royal College of Physicians, May 1826. By Francis Hawkins, M.D.

Nearly ready for publication.

A Memoir on the Geology of Central France, including the Volcanic Formations of Auvergne, the Velay, and the Vivarais. By G. Poulett Scrope, Esq. F.R.S. &c.

No. IX. of the Zoological Journal; containing, with Articles on various departments of Zoology, some Account of the Life and

Writings

Writings and Contributions to Science of the late Sir T. Stamford Raffles.

H. T. De la Beche, Esq. has in the press,—A Tabular and Proportional View of the Superior, Supermedial and Medial (Tertiary and Secondary) Rocks; to contain a List of the Rocks composing each Formation; a Proportional Section of each, its General Character, Organic Remains, and Characteristic Fossils;—on one large sheet.

NEW PATENTS.

To Sir William Congreve, of Cecil-street, Strand, for a new motive power.—Dated the 8th of February, 1827.—6 months allowed to enrol specification.

To William Stratton, of Limehouse, engineer, for an improved apparatus for heating air by means of steam.—12th of February.—6 months.

To John George Prist, of the Old City Chambers, Bishopsgate, for certain improvements, communicated from abroad, in copper and other plate printing.—14th of February.—6 months.

To Philip Jacob Heisch, of America-square, for improved machinery for spinning cotton, communicated from abroad.—20th of February.—2 months.

To Charles Barwell Coles, late of Duke-street, Manchester-square, esquire, and William Nicholson, of Manchester, civil engineer, for a new method of constructing gasometers, communicated from abroad.—20th of February.—6 months.

To William Benecke, of Deptford, in behalf of M. W. Pescatore, of Luxemburgh, for a machine for crushing seeds and other oleaginous substances, for the purpose of extracting oil therefrom.—20th of February.—6 months.

To William Jefferies, of London-street, Radcliffe, brass-manufacturer, for improvements in calcining or roasting and smelting or extracting metals from ores, &c.—20th of February.—6 months.

To Pierre Erard, of Great Marlborough-street, musical instrument-maker, for improvements in the construction of piano-fortes, communicated from abroad.—20th of February.—6 months.

To Augustus Count De la Garde, of St. James's-square, for a method of making paper from the ligneous parts produced from certain textile plants in the process of preparing them by the patent rural mechanical brake, and which substances are to be employed alone or mixed with other suitable materials in the manufacture of paper.—20th of February.—6 months.

To William Smith, of Sheffield, for an improved method of manufacturing cutlery and other articles of hardware by means of rollers.—20th of February.—6 months.

To Joseph F. Ledsam, of Birmingham, for purifying coal gas by means not hitherto used.—2nd of March.—6 months.

To Jonathan Lucas, and Henry Ewbank, both of Mincing-lane, for an improved process for dressing of paddy or rough rice.—10th of March.—2 months.

To

To Lemuel Wellman Wright, of Upper Kennington-lane, Surrey, engineer, for improvements in machinery for making metal screws.—17th of March.—6 months.

To Benjamin Rotch, of Furnival's Inn, esquire, for his diagonal prop for transferring perpendicular to lateral pressure.—22nd of March.—6 months.

To James Stewart, of Store-street, Bedford-square, piano-forte-maker, for improvements on piano-fortes, and in the mode of stringing the same.—22nd of March.—6 months.

To James Woodman, of Piccadilly, perfumer, for his improvement in shaving and other brushes.—22nd of March.—6 months.

To James Perkins, of Fleet-street, engineer, for improvements in the construction of steam-engines.—22nd of March.—6 months.

AURORA BOREALIS.

At six o'clock on the evening of the 13th at Gosport, a light appeared about the magnetic North, which increased in brilliancy and gradually extended at each end for several hours. The increasing of the arch of light, with some faint coruscations from its vertex between seven and eight o'clock, determined it to be the Northern Lights. Soon after eight the light reached to the two bright stars *Beta* and *Gamma* in the head of *Draco*, an altitude of 14 degrees. By half-past nine the eastern end of the light had extended to the N.E. by N. point of the horizon; its extension towards the East was gradual, as at first it was contained between the N. and N.W. by W. points. Soon after nine it was at its greatest extent; viz. upwards of 90 degrees, and the stars α Lyræ (*Vega*) and α Cygni (*Deneb*) were the most conspicuous in the Aurora. From a quarter before ten till about ten minutes past, the Aurora Borealis appeared in its greatest splendour, and it would have been more beautiful had not a dark *cirrostratus* cloud sprung up and intercepted a great part of it. At this time the northern hemisphere was in a blaze, and no atmospheric phænomenon could exceed the grandeur of the pale red columns suddenly emanating from circular patches of intense light, which broke out in almost every part of the arch, like eruptions from a volcano; some of these perpendicular columns were short, where the patches were small, and the others so long as to reach to within two or three degrees of *Polaris*, an altitude of 48 degrees from the northern horizon. The height of these electrical columns on mixing with the superior stratum of air caused several accensions or falling meteors, which are common accompaniments of the vivid Northern Lights. By half-past ten the clouds had extended nearly all over the Aurora; yet it was distinguished between them at intervals till half-past eleven, half an hour after the moon had risen. There was no appearance of it on the subsequent evenings. A brisk gale came on soon after this phænomenon, but did not arrive at its greatest force till the 22nd. The last Aurora Borealis observed here, was near midnight on the 25th of March 1821.

At 8 o'clock in the evening of the 17th of February, a bright light appeared 10 degrees above the northern horizon, and 20 degrees on each side of the magnetic North; and from a quarter past 9 till twenty minutes to 10 o'clock, several vivid patches of light appeared at intervals in the Aurora, from which perpendicular columns emanated; but their altitudes could not be determined, in consequence of intervening black clouds, from which at 10 o'clock a sprinkling of snow descended, and the Aurora rather suddenly.

disappeared

disappeared. The star α Cygni (*Deneb*) was conspicuous in the Aurora; and two small meteors fell immediately after the last of the coruscations had ascended. The thermometer rose about 3 degrees during the time of the Aurora, also in the evening of the 18th of January, when it appeared, which indicates a diffusion of warmth through the atmosphere from this electrical phænomenon.

In 12 hours after its disappearance a strong gale came on from the East, and continued about 40 hours.—An article on the recent Northern Lights from a higher north latitude than this, would be acceptable by the way of comparison.

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·44 Feb. 3. Wind NE.—Min. 29·49 Feb. 28. Wind SW.
Range of the mercury 0·95.

Mean barometrical pressure for the month	29·983
——— for the lunar period ending the 25th instant	29·996
——— for 14 days with the Moon in North declination . .	30·100
——— for 15 days with the Moon in South declination . .	29·892
Spaces described by the rising and falling of the mercury . . .	5·070

Greatest variation in 24 hours 0·480.—Number of changes 16.

Therm. Max. 56° Feb. 27. Wind SW.—Min. 14° Feb. 16. Wind E.
Range 42°.—Mean temp. of exter. air 36°·62. For 30 days with ☉ in ♒ 35·27
Max. var. in 24 hours 21°·00—Mean temp. of spring water at 8 A.M. 49°·12

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 28th	98°
Greatest dryness of the air in the afternoon of the 18th . . .	45
Range of the index	53
Mean at 2 P.M. 58°·6—Mean at 8 A.M. 66·3—Mean at 8 P.M.	66·1
—— of three observations each day at 8, 2, and 8 o'clock . .	63·7

Evaporation for the month 0·90 inch.
Rain near ground 0·820 inches.—Rain 23 feet high 0·765 inches.
Prevailing Wind N.E.

Summary of the Weather.

A clear sky, 4½; fine, with various modifications of clouds, 11½; an overcast sky without rain, 10; rain, 2.—Total 28 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
8	6	24	0	14	4	8

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
	13½	4	1	1	3½	½	2	28

General Observations.—The weather this month has been remarkably dry, with a long continuance of a piercing North-east wind, and extremely cold for the advanced state of the winter quarter. Persons of delicate constitutions have felt this wind, which prevailed about half the month, very searching. No rain fell here from the 1st to the 26th, only light sprinklings of snow on the 3rd, 12th, 15th, and 17th, which were not measurable when dissolved. From the 9th to the 21st the fields, gardens, ponds and marshes were ice-bound, and the carriage roads as dusty as is often seen in March.

On

On the 16th the thermometer receded 18 degrees below the freezing point, and the night was the coldest we have had here since the 15th of January 1820, when the same degree of cold was registered. The *minimum* temperature of the external air at Paris in the nights of the 17th and 18th instant, was four degrees of Fahrenheit's scale lower than it was here on the 16th, Paris being about 110 miles inland from the nearest sea-shore of the British Channel.

This depression of temperature, though it came on rather late, was very seasonable, as it will prove beneficial to cultivated lands, and in a great measure preserve the corn and vegetation, in consequence of the destruction of ground insects, which for the last two or three years had increased and made such ravages thereon. The thaw was so slow and gradual from the 21st to the 25th, as to be almost imperceptible; but on the 27th the thermometer rose to 56 degrees, with a South-west gale and a humid air.

The mean temperature of the external air this month, is 2·3-10ths degrees colder than that of January, and 5·1-10th degrees colder than the mean of February for the last eleven years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one Aurora Borealis, four solar halos, four meteors, and ten gales of wind, or days on which they have prevailed, viz. six from the NE., one from E., and three from the S.W.

REMARKS.

London.—Second month. 1. Cloudy. 2. Some snow in the evening, more during the night. 3. Fine. 4. Cloudy. 5. Cloudy. 6. Cloudy: drizzly. 7. Fine. 8. White frost: fine day. 9. Fine. 10. Fine. 11. Cloudy. 12. Slight showers of snow during the day. 13. Fine. 14. Cloudy: a little snow. 15. Fine: with occasional snow showers. 16. Fine: bleak. 17. Hoar frost: foggy morning: fine afternoon. 18. Fine. 19. Cloudy. 20. Cloudy. 21. Cloudy: some snow in the afternoon. 22. Fine. 23. Foggy morning: cloudy. 24. Foggy morning: fine day. 25. Hoar frost: fine afternoon. 26. Thaw commenced this morning with gentle rain: a very stormy night succeeded. 27. Boisterous wind in the morning: rainy afternoon. 28. Rainy: very boisterous night.

RESULTS.

Winds, N. 2: N.E. 10: E. 3: S.E. 3: S.W. 3: N.W. 6: var. 1.

Barometer mean height for the month	30·239 inch.
Thermometer, mean height for the month..................	32·625°
Evaporation	1·32 inch.
Rain	·88 inch.

Boston.—Feb. 1. Rain. 2, 3. Fine. 4. Cloudy. 5. Fine. 6, 7. Cloudy. 8. Fine. 9. Cloudy. 10. Fine. 11, 12. Cloudy. 13. Snow. 14. Cloudy. 15. Fine. 16. Snow. 17. Fine. 18. Cloudy. 19. Fine. 20. Cloudy: hail storm. 21. Cloudy. 22. Fine. 23—25. Cloudy. 26. Cloudy: rain A.M. 27. Fine. 28. Cloudy: rain at night.

Penzance.—Feb. 1. Misty: rain. 2—4. Clear. 5. Fair: clear. 6. Cloudy. 7. Fair: clear. 8, 9. Clear. 10—16. Fair. 17. Light showers: fair. 18. Snow showers. 19. Fair: clear. 20, 21. Fair. 22—24. Clear. 25, 26. Rain. 27. Showers. 28. Rain.—Rain gauge *ground level.*

Meteor-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8 A.M.										
Feb. 1	30·10	29·95	29·50	29·44	29·70	29·68	29·50	48	34	47	45	49	38	47	SW.	SE.	E.	calm	...	...	...	...	0·070	0·27
2	30·58	30·10	29·80	29·70	30·00	29·80	29·75	40	27	41	39	42	31	33·5	NE.	E.	NE.	E.	...	...	...	0·300	...	·20
☽ 3	30·75	30·58	30·30	30·10	30·44	30·25	30·23	36	26	39	32	37	30	32	NE.	E.	NE.	E.	...	0·10	...	...	...	...
4	30·75	30·69	30·30	30·20	30·44	30·44	30·40	38	33	39	29	42	31	34·5	NE.	E.	NE.	calm	...	...	...	...	...	...
5	30·69	30·48	30·12	30·10	30·34	30·31	30·35	40	23	38	31	40	27	29·5	E.	E.	NE.	NE.	...	...	...	...	...	...
6	30·60	30·48	30·00	30·00	30·24	30·22	30·12	40	34	40	28	40	33	33·5	N.	E.	NE.	NW.	...	·15	...	...	...	...
7	30·64	30·60	30·10	30·10	30·34	30·33	30·25	40	24	40	34	43	31	37·5	NE.	E.	NE.	calm	...	...	...	...	...	...
8	30·60	30·57	30·12	30·12	30·36	30·35	30·33	[illegible]	27	38	31	38	30	35	SE.	E.	NE.	E.	...	...	...	...	...	...
9	30·57	30·37	30·08	30·00	30·26	30·20	[illegible]	[illegible]	[illegible]	40	29	38	31	36	NE.	E.	NE.	E.	...	·10	...	...	...	...
10	30·37	30·08	29·90	29·80	30·09	29·97	[illegible]	[illegible]	[illegible]	37	30	41	34	35	NE.	E.	NE.	E.	...	...	...	...	...	...
○ 11	30·08	30·08	29·70	29·68	29·80	29·74	29·73	36	[illegible]	36	32	35	33	35	NE.	NE.	NE.	E.	...	...	...	...	...	...
12	30·30	30·08	29·78	29·68	30·00	[illegible]	[illegible]	41	[illegible]	[illegible]	32	43	32	37·5	NE.	NE.	N.	calm	...	·10	...	...	...	...
13	30·30	30·26	30·08	29·95	30·18	[illegible]	29·90	[illegible]	[illegible]	44	35	42	29	32	SW.	NE.	N.	NE.	...	...	...	...	...	...
14	30·26	30·09	29·96	29·90	30·10	[illegible]	[illegible]	40	[illegible]	41	35	44	35	34	NW.	N.	NW.	calm	...	...	...	...	...	...
15	30·35	30·12	29·84	29·80	29·98	29·87	[illegible]	39	16	[illegible]	38	43	24	33·5	NW.	NE.	NW.	calm	...	·14	...	...	...	...
16	30·35	30·17	29·80	29·80	30·10	30·05	[illegible]	[illegible]	11	[illegible]	30	[illegible]	14	27	var.	E.	E.	calm	...	...	...	...	...	...
17	30·29	30·16	29·72	29·70	29·94	[illegible]	[illegible]	29	[illegible]	37	29	35	24	24	NW.	E.	W.	calm	...	...	...	...	...	...
18	30·29	30·17	29·55	29·48	29·94	29·91	[illegible]	32	10	38	25	33	26	28	E.	SE.	E.	E.	...	·05	...	...	...	...
☾ 19	30·17	29·95	29·48	29·46	29·79	29·69	[illegible]	[illegible]	[illegible]	38	27	35	27	26	E.	NE.	NE.	E.	...	...	...	0·200	...	...
20	29·95	29·89	29·40	29·40	29·62	29·60	29·67	[illegible]	20	37	31	34	32	30	NE.	E.	NE.	E.	·85	...	...	...	...	...
21	30·07	29·89	29·50	29·38	29·68	29·57	[illegible]	36	[illegible]	40	33	41	32	32·5	NE.	NE.	NE.	E.	...	·05	...	...	...	·28
22	30·27	30·07	29·80	29·70	30·03	29·83	29·70	40	22	44	32	42	25	31	N.	NE.	N.	NW.	...	...	...	...	...	...
23	30·27	30·15	29·88	29·80	30·07	29·97	29·87	40	20	47	28	43	35	30	NW.	NE.	NW.	calm	...	...	...	...	...	...
24	30·37	30·15	29·70	29·70	30·02	29·92	29·73	44	14	41	33	43	29	33·5	NW.	SE.	N.	calm	...	·09	...	...	...	...
● 25	30·37	30·08	29·75	29·70	30·11	30·02	30·00	43	33	49	34	45	36	34	SE.	SE.	E.	calm	...	...	...	...	...	...
26	30·08	29·70	29·60	29·40	29·84	29·74	29·50	41	39	52	40	54	45	37	SW.	SW.	SW.	SW.	...	...	·13	0·750	0·060	...
27	29·88	29·70	29·48	29·38	29·69	29·56	29·05	55	37	51	48	56	47	52	SW.	W.	SW.	W.	...	...	...	...	...	·06
28	29·70	29·52	29·38	29·20	29·61	29·49	29·50	57	41	52	45	51	46	35	SE.	SW.		calm	·47		·75	0·350	...	...
Aver.:	30·75	29·52	30·30	29·20	30·44	29·49	29·87	57	10	54	27	56	14	33·71					1·32	0·90	·88	1·600	0·820	0·81

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MAY 1827.

LXII. *On the Orange Phosphate of Lead. By the Rev.* WM. V. VERNON, *F.R.S. Pres. Y.P.S.**

IN a former communication which I had the honour of making to the Society, I stated as the result of some analytical researches into the cause of the diversity of colour in the phosphates of lead, that I had found manganese in the green phosphate, and chrome in the orange phosphate from Wanlock head; at the same time I offered some reasons for supposing that the chrome in this mineral is in the state of protoxide.

Such a supposition, however, afforded no satisfactory solution of the orange colour of the phosphate; for though many of the combinations of the chromic acid are yellow or red, and especially its combination with lead, this is not the case with the green oxide of chrome.

For the purpose of clearing up this difficulty, I have lately resumed the inquiry, and made the experiments, of which I now propose to give the Society an account.

Upon sixty grains of the mineral I poured nitric acid a little diluted with water, which with the assistance of heat dissolved the whole, except four-tenths of a grain, consisting of silex and red oxide of iron. The colour of the solution was a golden yellow.

Sulphuric acid was now added to precipitate the lead. The sulphate of lead weighed 63·4 grains. The liquid after the separation of the lead retained its yellow colour.

I took a third part of this liquid to examine the nature of its contents, reserving the other two-thirds for the determina-

* Read to the Yorkshire Philosophical Society, March 6, 1826; and communicated by the Author.

tion of quantities, and I added to it caustic soda; a precipitate fell which had a greenish tinge; the liquid also changed its colour to a yellow green, and being boiled let fall a further green precipitate. These precipitates consisted of the protoxide of chrome, together with some lead and lime.

The liquid after the separation of the last precipitate resumed its yellow colour, which led me to imagine that it might contain chromic acid. The method which I adopted for ascertaining this point, was founded upon the property possessed by certain vegetable acids of abstracting oxygen from the acid of the chromates, a property not belonging to the acetic or prussic acids, but which I have found in the oxalic and citric as well as in the tartaric. The two latter of these, though they convert the chromic acid into protoxide of chrome, do not furnish a ready method of separating the chrome, because a part of them remaining undecomposed, they form soluble triple salts with the oxide and with whatever alkali might be employed to throw it down; but if the oxalic acid be used, though in this case also the chrome cannot be thrown down by ammonia, it may, with the assistance of heat, be precipitated by soda.

I therefore added oxalic acid to the liquid, and after boiling it perceived a change in the colour: having then neutralized it with soda, I obtained a green precipitate; this was separated and re-dissolved in oxalic acid, to clear it from any lime that might have fallen with it; a small quantity of lead still adhering to it was removed by sulphuretted hydrogen, it was neutralized with soda; and of the precipitate which took place a particle was heated on platina foil with nitrate of potash: the yellow salt thus obtained gave with the nitrates of silver and lead the crimson and yellow precipitates by which the chromates are distinguished.

It is evident from these experiments that chromic acid existed in the nitric solution of the mineral; and since nitric acid does not, in the circumstances above described, acidify the protoxide of chrome, it may be concluded that the orange phosphate of lead contains chromic acid, and the colour of the mineral is thus sufficiently accounted for.

A good reason can now be assigned for the circumstance noticed by Klaproth, that when the muriate of tin is poured upon this phosphate it removes the colour,—a phænomenon which induced him to suppose that the lead contained in it is in a state of superoxidation. We see also how it happens that when the crystals of this substance are heated by the exterior flame of the blowpipe, or out of contact with inflammable matter, the colour is unaltered, growing only darker during ignition;

ignition; whereas when they are heated in the interior flame, they become green by the reduction of the chromic acid.

In considering the circumstances of this analysis, it appeared to me extraordinary that the chrome should be partly in the state of protoxide and partly of chromic acid; and to this point I directed my attention in examining the remainder of the nitric solution. Instead of neutralizing it, as before, I now evaporated it down, and observed the yellow colour change by degrees to green; ammonia was then added, and the whole of the chrome was thrown down, no chromic acid being left in solution.

Nitric acid alone has no tendency to reduce the acid of chrome. The mineral, therefore, must have contained something which might contribute to this reduction. I heated some of the crystals in a glass tube, and observed a strong empyreumatic smell indicative of vegetable matter. I then dropped minute portions of sugar, green vegetable substances, oil of turpentine and bituminous coal, into solutions of the bichromate of potash in nitric acid; by each of these, when heat was applied, the chromic acid was reduced. I do not find this effect to be produced by the gases which the action of the nitric acid on vegetable matter evolves when they are passed through such a solution: nitrous gas does not affect the bichromate of potash, neither does hydrogen in the gaseous state, nor carburetted hydrogen; it is decomposed, indeed, by the gaseous products of the distillation of sulphuric acid and alcohol, but this reduction is not effected by the olefiant gas, but by a portion of sulphurous acid which accompanies it.

Another circumstance in the constitution of the mineral may contribute to the reduction of the chromic acid. Klaproth has stated that the yellow phosphate of lead contains a portion of muriatic acid; and I have found it in the specimen which I have analysed. Now muriatic acid reduces the chromic acid when heated with it; and in the present case, the muriate of lead having been decomposed by the sulphuric acid, the muriatic acid would be free to operate.

Upon the whole, there is no reason to doubt that the chrome is here united to the phosphate of lead, in the state of chromic acid, or rather of chromate of lead. When the mineral is dissolved in nitric acid and heated, a portion of the chromic acid is reduced, by one or both of the causes which I have stated: if it is then neutralized with an alkali, part of the chrome is thrown down as protoxide, and part remains in solution as a chromate; but if the nitric solution is evaporated without having been neutralized, the whole of the chrome is reduced, and may be precipitated in the state of protoxide.

 I have

I have taken for granted, from the general account given by Klaproth of the yellow phosphate which he examined, and of its locality, that it was the same mineral as that of which I have now been giving an account, though he calls its colour citron yellow, and though his description of its crystalline character is defective; for in the more perfect specimens the form of the crystal is a regular six-sided prism. It is not to be wondered that he should have overlooked the chrome,—a substance of which, when he made his analysis, nothing I believe was yet known, and which is here present in a very minute proportion; not more, if my experiments are correct, than between five- and six-tenths of a grain of the protoxide in a hundred of the mineral.

The amount of oxide of lead which Klaproth found, and with which my analysis nearly agrees, was eighty per cent. If from this the proper deduction be made for the chloride of lead calculated from his statement of the muriatic acid which the mineral contains, and also for the chromate of lead which I have found in it, the phosphoric acid which corresponds with the remaining oxide of lead will be somewhat less than it is given by Klaproth: but the quantity of phosphoric acid can scarcely be obtained with accuracy by the method which he employed, in precipitating it with lead. Thus calculated, the composition of the mineral may be stated in the following proportions:

Phosphate of lead	87·66
Chloride of lead	10·07
Chromate of lead	01·20
Water and combustible matter . .	00·40
Silex, lime, red oxide of iron . . .	00·67
	100·00

LXIII. *Some Remarks on a Memoir by* M. Poisson, *read to the Academy of Sciences at Paris, Nov.* 20, 1826, *and inserted in the* Conn. des Tems 1829. *By* J. Ivory, *Esq. M.A. F.R.S.**

Magna vis veritatis.

In the *Conn. des Tems* 1829, lately published, there is inserted a long Memoir by M. Poisson, on the attraction of spheroids. The intention of it is, to vindicate the theory of the figure of the planets contained in the *Méc. Céleste,* from all the objections that have been urged against it. The talents

* Communicated by the Author.

and

and acquirements of the author of the Memoir are well known: he has made this branch of the mechanical philosophy more particularly his study, and has applied the peculiar kind of analysis employed in it to different problems; so that among existing mathematicians an abler vindicator could not have been found. Every subject that passes through such hands must acquire valuable improvements; and if, on the present occasion, M. Poisson has not succeeded in removing every difficulty, this must be ascribed to the doctrine he defends, which can never be entirely freed from inconsistency, nor perfectly reconciled to clearness and accuracy of demonstration.

The author begins his Memoir with stating anew the fundamental principle of the analytical theory: he then repeats the demonstration of it he had given on a former occasion, and endeavours to defend it against an objection advanced in the Phil. Mag. for January 1826, p. 37. It is chiefly on this part of M. Poisson's paper, extending through about four pages, and another short passage, that I intend to offer some brief remarks. Supposing that the reader has the Memoir alluded to before him, I shall, for the sake of abridging, write y and y' for $\varphi(\theta, \psi)$ and $\varphi(\theta', \psi')$, and f for $\sqrt{1 - 2\alpha p + \alpha^2}$: I shall also write ds for the differential of the surface of the sphere; it is equal to $\sin\theta' d\theta' d\psi'$, when its position is determined by the arcs θ' and ψ'; and it may be similarly expressed by any other two independent arcs that fix its place, if any transformation should make this convenient. For the sake of simplicity, I shall further suppose that α never exceeds 1, although it is always near it, and approaches it as a limit. The formula (3), p. 330, will then be thus written,

$$X = \frac{1}{4\pi}\int \frac{(1-\alpha^2)\, y'\, ds}{f^3}.$$

Now the fluent being extended to the whole surface of the sphere; or, which is the same thing, the integration being effected separately for the two variables θ' and ψ', from $\theta' = 0$, $\psi' = 0$ to $\theta' = \pi$, $\psi' = 2\pi$; it is proposed to prove that $X = y$, in the particular case when $\alpha = 1$.

The distinguishing features of the formula are these: the numerator is always inconsiderable, because $1 - \alpha^2$ is small; and, taking θ and ψ for the initial values of θ' and ψ', the denominator increases rapidly from the least value $(1 - \alpha)^3$, so as to become incomparably greater than the numerator when the two variable arcs have acquired very small increments. On these grounds M. Poisson thinks himself entitled to integrate on the assumption that y' does not vary from the initial value y: then,

$$X = \frac{y}{4\pi}\int \frac{(1-\alpha^2)\, ds}{f^3}$$

The

The difficulty is now overcome; for there is no doubt that the integral is equal to 4π. The integration may be performed as usual; or it may be accomplished by the peculiar process of M. Poisson. The argument cannot be affected by different algebraic operations that lead to the same result. But we may fairly hesitate to admit the gratuitous supposition of making y' constant. In order to examine this point, put $y' = y + (y' - y)$*: then

$$X = \frac{y}{4\pi}\int\frac{(1-\alpha^2)\,ds}{f^3} + \frac{1}{4\pi}\int\frac{(1-\alpha^2)(y'-y)\,ds}{f^3}.$$

If we neglect the term newly introduced, what remains is M. Poisson's demonstration. Whether we admit or reject his conclusion, will therefore depend upon the evidence we have that the term omitted is evanescent. Now put this term equal to zero; separate it into the two parts of which it consists; and substitute the known value of the integral multiplying the constant quantity y: then,

$$y = \frac{1}{4\pi}\int\frac{(1-\alpha^2)\,y'\,ds}{f^3}.$$

But this is neither more nor less than the original formula to be demonstrated, if we substitute y for X. It appears, therefore, that the very property to be proved is involved in the omitted term; or, which is the same thing, in the assumption made by M. Poisson, that y' is constant. The boasted demonstration published in 1823†, which was to dissipate all doubts and objections, is merely a *petitio principii.* I am induced to make such observations, because I am concerned to show that the objections I have made are not frivolous, but such as it would be a reproach to any one to overlook them in the profest examination of a difficult question.

But, in his new Memoir, M. Poisson endeavours to correct his former demonstration, by considering the term which must be taken into account, in order to confer rigour and accuracy upon the reasoning‡. To use a homely phrase, he makes no bones of it. He resorts to his former assumption, and integrates on the supposition that $y' - y$, or ζ as he denotes it, is an infinitely small constant quantity. By this means the term in question comes out infinitely small, or zero: and this is all which is thought necessary for settling the point in dispute.—Will this pass for demonstration? It is a mere assertion. It is one of those curt and *imperative* attempts at proof, of which too many occur in the modern mathematics, which are none

* Phil. Mag. for Jan. 1826, pp. 36, 37.

† *Journal de l'Ecole Polytechnique*, 19^{e} cahier, p. 145.

‡ *Conn. des Tems* 1829, pp. 332, 333.

of its improvements, and which ought never to be admitted without scrupulous examination. In reality, the procedure of M. Poisson hides, from the attention of his readers, the true principles of the case. The numerator and the denominator of the expression vanish together; and the value of the fluent will depend entirely on the limit of the ratio of the two quantities as they both approach zero. According to that value, the fluent may be evanescent, or it may be finite, or infinitely great.

It is remarkable that the analytical process employed by M. Poisson, if he had pursued it accurately, would have led him to a right result. Put $\theta' = \theta + h$, $\psi' = \psi + k$; then according to the operations in p. 331, the term of which the value is sought, will take this form, viz.

$$\frac{1}{2\pi}\int \frac{g(y'-y)\sin\theta\, dh\, dk}{(g^2+h^2+k^2\sin^2\theta)^{\frac{3}{2}}}.$$

But as y' varies with h and k, we must not make $y'-y$, or the ζ of M. Poisson, constant. We have

$$y'-y = \frac{dy}{d\theta}\cdot h + \frac{dy}{d\psi}\cdot k = \text{A}h + \text{B}k,$$

putting A and B for the differential coefficients. By substitution, our expression will become,

$$\frac{1}{2\pi}\int \frac{g\,\text{A}\sin\theta\, h\, dh\, dk}{(g^2+h^2+k^2\sin^2\theta)^{\frac{3}{2}}}$$

$$+\frac{1}{2\pi}\int \frac{g\,\text{B}\sin\theta\, dh\, k\, dk}{(g^2+h^2+k^2\sin^2\theta)^{\frac{3}{2}}}.$$

Here the two parts are similar, and the integrations are readily performed by the procedure of M. Poisson: the result is this,

$$\frac{g}{2\pi}\left\{\text{A}\log\frac{\sqrt{g^2+h^2}}{g} + \frac{\text{B}}{\sin\theta}\log\frac{\sqrt{g^2+k^2\sin^2\theta}}{g}\right\}.$$

Although g is a vanishing factor, we must not immediately infer that the whole of this expression is always evanescent. It is necessary to take into account the ultimate values of h and k which again depend upon the limit of $\frac{y'-y}{f}$. If we suppose that $y'-y$ is ultimately divisible by f^2, it is manifest that the expression is evanescent, which proves M. Poisson's proposition for such functions. This is the only case comprehended in Laplace's demonstration, *Méc. Céleste*, liv. xi. pp. 25, 26. Again, if we suppose that $y'-y$ is ultimately divisible by f, the quantity multiplied by g will be finite; the whole expression will therefore be equal to zero; and this proves the proposition for all rational functions of $\cos\theta$, $\sin\theta\,\cos\varphi$,

sin

$\sin\theta \sin\psi$, which possess the supposed property. In all other cases the value of the above expression is indeterminate, and the demonstration of M. Poisson's formula, or, which is the same thing, of Laplace's fundamental equation in partial differentials, ceases to be exact.

Mr. Professor Airy, in a short paper read to the Cambridge Philosophical Society in May last, and printed in their Transactions, has treated of this subject; and he advances rather a singular opinion. He agrees with me that the method of Laplace must be limited to a particular class of spheroids; and he claims the honour of having first placed the matter in its true light. But he attempts to show that the fundamental equation, *Méc. Céleste*, liv. iii. No. 10, is exactly demonstrated. Now admitting that the equation in question is accurately and *numerically* proved, it seems impossible to deny that the series of terms deduced from it, is *numerically* equal to the distance between the surfaces of the sphere and spheroid. I have always contended that the fault lay in the supposed generality of the equation, which is true only in a particular class of spheroids. On the other hand MM. Laplace and Poisson have upheld the universality of the equation by new proofs, of which I have here had occasion to speak. In my view the theory is freed from its difficulties, and becomes satisfactory, although stript of its high pretensions to generality. Mr. Professor Airy, by supporting the fundamental equation without restricting it, and at the same time denying the unavoidable consequence, has only introduced new inconsistencies, and embroiled, with new difficulties, a subject very seducing by its analytical elegance, but very perplexing when we resolutely seek to exhibit to the understanding a rational account of its principles.

In examining the theory of Laplace, the want of rigour in the analysis could hardly escape detection; and in a subject of so great interest and difficulty, it seemed requisite to scrutinize and clear up every doubtful point. But the nature of the analysis will become a consideration of only secondary importance, if it shall appear that there are defects in the first principles, or in the conditions of equilibrium. In the problem of the figure of a planet in a fluid state, there are too different cases; for we may suppose it to consist of only one homogeneous fluid, or of several fluids arranged in strata varying in density from the centre to the surface. If the first case were solved, the theory of equilibrium of which we are in possession, would be sufficient for investigating the second case. But the present theory fails in the equilibrium of a homogeneous planet. I have found that the equilibrium cannot take

take place unless two conditions, or laws, which I need not here repeat, are both fulfilled, of which one only is necessary according to the usual doctrine*.

There is a remarkable proof of the deficiency of the usual theory in M. Poisson's Memoir. He applies his analysis to a homogeneous fluid mass, revolving upon an axis, and nearly spherical†. When the square of the centrifugal force is neglected, he finds that the figure of the fluid must be an elliptical spheroid, agreeing with the solutions of Legendre and Laplace. But, on attempting to carry the approximation further, the method fails; all that can be known is, that there is only one figure which will satisfy the equations:—*Mais ce procédé ne sauroit déterminer davantage ce solide*‡. Now, what is the reason of this? It cannot be the want of mathematical methods; for the symbols are all arranged, and ready to obey the directions of the analyst. The truth is, there is no principle to govern the calculation after the first step. The machinery is sufficient, and ready prepared; but it cannot be set to work, because there is no fulcrum for its support. In order to supply the defects of his method, M. Poisson has recourse to the elliptical spheroid, which is known to satisfy the conditions of the problem; and he infers that his series for the radius of the solid, must coincide with the expansion of the radius of the ellipsoid§. Now it is far from clear that he is right in this inference. If I take in both my conditions, and thence deduce the resulting figure of equilibrium, there is no doubt that the radius, to whatever length the expansion is carried, will coincide with an elliptical spheroid; because this is the only figure deducible from the premises. But, if I leave out one of my two conditions, and attempt to solve the same problem by means of the other alone, which is exactly what M. Poisson has done, it is next to certain that the new computation will not agree with the former one.

There are no direct objections to my theory; but it stands

* In the Phil. Trans. 1826, p. 557, there is a note of Mr. Airy, very injurious to me. He is treating of spheroids of variable density, and evidently misapprehends my conditions of equilibrium, which I have always limited to the case of homogeneity. The R. S. are not responsible for the accuracy of what they publish: but I apprehend few instances will be found so injurious to an individual, cast upon the public on the authority of mere assertion, and arising from mistaken notions. But I console myself because I know with the certainty of demonstration, that Mr. Airy's problem, admitting that any practical utility could be attached to it, is not solved, and that it cannot possibly be solved except by my theory, and indirectly, with the help of that law with which he so flippantly finds fault. What a difference between the supercilious importance of the Cambridge Professor, and the candid expositions of M. Poisson!

† *Conn. des Tems* 1829, p. 371. ‡ Ibid. p. 373. § Ibid. p. 375.

 opposed

opposed to the splendid analytical processes that have been so long and so unsparingly admired. According to my view, there can possibly be but one figure of equilibrium of a homogeneous planet in a fluid state; and in fact, this comprehends all that geometers have been able to accomplish in this question. The usual theory advances one step in one particular case; and then it leaves the geometer in the lurch, without his being able to explain the reason of the failure. Beyond this it has been entirely inefficient:—*Quand la masse fluide n'est pas assujettie à differer très peu de la sphère, les géomètres n'ont point encore determiné l'espèce de figure qui satisfait à l'equation d'equilibre**. In an elliptical spheroid *in equilibrio*, it is known that the rotatory velocity is limited, being contained between zero and a maximum quantity; so that there are two different figures that have the same rotation. On this ground M. Poisson makes an objection, which I notice the more willingly, because it does not turn upon any technical point of analysis. *Si l'ellipsoide etait la seule figure qui eut cette propriété, il en resulterait cette consequence singulière, que l'equilibre serait impossible pour une rapidité de la rotation, qui n'est cependant pas celle ou le fluide commencerait à se dissiper**.

Suppose a homogeneous mass of fluid, at rest, *in equilibrio*, and consequently spherical in its figure: conceive a great circle of the sphere extending indefinitely, and an axis, or diameter, perpendicular to the great circle. Now let a velocity of rotation about the axis be communicated to the fluid sphere: I impose no restriction to the degree of the velocity, except that it must not be such as to dissipate the particles, which must retain their continuity. The rotatory motion will cause the fluid to recede from the axis, and to subside at the poles; and to these effects there would be no limit, if the centrifugal force were not opposed by that part of the attraction of the particles which is directed perpendicularly to the axis. At a certain degree of oblateness the two opposite forces will be equal; and although the recession of the fluid from the axis will not immediately cease at this point, yet it will soon be entirely arrested. The figure of the fluid will now return in an opposite direction, becoming less oblate, and passing a little beyond the limit at which the two forces are equal. The fluid will thus oscillate about a state of equilibrium; and if we admit any tenacity or friction of the particles, the oscillations will gradually decrease, and finally settle in a permanent figure. But it is to be principally observed that, whenever the fluid

* *Conn. des Tems* 1829, p. 375.

recedes

recedes from the axis, the rotatory velocity will decrease; and whenever it returns in a contrary direction, the same velocity will increase; and ultimately, in the state of equilibrium, the rotation actually remaining, will depend upon the nature of the figure of equilibrium, and the proportion of the two forces urging the particles. Although we suppose that the rotation *in equilibrio* is small, yet we cannot infer that the rotatory velocity originally imprest, was likewise small. On the contrary, if the rotation were very small, and at the same time the figure very oblate, we must conclude that the primitive rotatory force was just within the limit required to dissipate the fluid. What particular figure the fluid *in equilibrio* will have, we do not now inquire; but we are entitled to infer that there is only one such figure for every degree of rotatory force originally communicated to the fluid sphere. This is incompatible with the usual theory; and it refutes M. Poisson's argument. But it is very consistent with my system; nay, it can be consistent with no other; for if there be but one figure, that, it is certain, must be an oblate elliptical spheroid.

But perhaps all this concurring evidence may not be sufficient to overcome the prejudice in favour of a spendid theory, very powerfully upheld from various motives. I hope soon to lay other more direct demonstrations before the public. But I have observed on a former occasion that this branch of science is discouraged and undervalued; and a passage in the last Quarterly Journal of Science*, written by a modern F.R.S. corroborates what I ventured to allege. The theory of the figure of the planets originated with Newton and Huygens: it has been the subject of incessant discussion for a century; it has been attended with greater difficulty, and has occasioned a greater number of memoirs, than any other branch of the system of the world. It has occupied the attention of Clairaut, Maclaurin, D'Alembert, La Grange, Legendre, Laplace, and Poisson: and I shall not easily be brought to think slightly of the speculations of such men, even when compared with the bustling activity in philosophical pursuits that now prevails. One can hardly help thinking that, in order to make amends for past remissness, the indefatigables of the present day are now determined to take Nature by storm. In allusion to what is said, in the passage cited, respecting the studies to which I have been attached, but in allusion to that only, I shall close these remarks with declaring, that I am prouder of the strictures of such a critic, than I should have been of his praise.

April 4, 1827. J. Ivory.

* Page 17.

 LXIV. *On*

LXIV. *On Capillary Attraction.* By the Rev. J. B. Emmett.*

[Continued from p. 118.]

THE force which elevates a column of liquid in a fine tube, between two plain surfaces, or around solid matter generally, is corpuscular: the effect is produced by either the surface only of the solid, or a stratum of immeasurably small depth. The liquid is elevated by the attraction of the solid to its upper strata: for, if the upper strata alone of the suspended column be heated, as great an effect is produced as by heating the whole column to the same temperature, which is apparent when the mechanical principles upon which the phænomenon depends are considered.

This fact I did not ascertain until very recently: I had gone through a long series of experiments, using a test tube which contains the liquid, into which is inserted the capillary tube, along which an index moveable by a fine screw, having a divided head, slides: the liquid having arrived at its proper altitude, the whole was plunged into water or other liquid heated to a given temperature, the index being a little above the surface. After having repeated a great number of experiments, many of which were anomalous, I found those which had occupied several weeks wholly useless. The whole series must be repeated by help of an apparatus, which will allow the summit of the column to be seen, and by which any required temperature may be applied to the upper part only. These experiments being useless, I was not able to communicate a paper for the last Number.

In the former paper, I showed that if the density of a liquid be changed by expansion or contraction, the altitude of the column is not affected. This may be proved experimentally:—Heat as much as possible of the capillary column, except the upper strata, and no sensible effect is produced: apply the same temperature to the upper part, and a notable depression takes place, such as 3 or 4 parts in 20; even the heat of the hand causes a depression of about $\frac{1}{30}$th of an inch in a column of $2\frac{2}{10}$ inches.

I now proceed to investigate the phænomena of such compounds as saline solutions, dilute acids, &c. Let there be two substances, A and B; let the compound be C.

Let $a =$ sp. gr. A $\quad b =$ sp. gr. B $\quad c =$ sp. gr. C
$\quad d =$ weight of A $\quad e =$ weight B $\quad f =$ weight C $= d + e$
$\quad$ H $=$ altitude to which A is elevated; $h =$ altitude B;
$\quad h' =$ altitude of C.

* Communicated by the Author.

The

The volume of A $= \frac{d}{a}$; volume of B $= \frac{e}{b}$; volume of C $= \frac{d+e}{c}$.

But since each substance A, B, is diffused throughout the whole volume,

$$\frac{d+e}{c} : \frac{d}{a} :: a : \text{density of A in its diffused state} = \frac{d\,c}{d+e} :$$

also $\frac{d+e}{c} : \frac{e}{b} :: b$: density of B in its diffused state $= \frac{e\,c}{d+e}$.

Hence, (Note †, p. 118, No. for February)

Force of A $=$ H $. \frac{d\,c}{d+e}$; force of B $= h . \frac{e\,c}{d+e}$; and the sum of these forces is equal to the force of the compound $h' c$; i. e.

$$\frac{\mathrm{H}\,d + h\,e}{d+e} = h' \quad \ldots\ldots\ldots\ldots \quad (a)$$

and

$$\frac{h' \{d+e\} - \mathrm{H}\,d}{e} = h \quad \ldots\ldots\ldots \quad (b)$$

But if, as generally happens, three tubes of different diameters Δ, δ, δ', are used respectively for the liquids A, B, C; then

$$h' = \frac{\Delta\,\mathrm{H}\,d + \delta\,h\,e}{\delta' \{d+e\}} \quad \ldots\ldots\ldots\ldots \quad (c)$$

Again: since the force of attraction is proportional to the altitude of the column, multiplied into the density of the liquid, in the same tube;

$$\text{Force of C} = h' c = \frac{\{\mathrm{H}\,d + h\,e\} . c}{d+c} \quad \ldots\ldots\ldots \quad (d)$$

By transposing the equations *c* and *d*, the forces of attraction may be found under all circumstances.

The primary formula (*a*) may be derived more simply, but not so satisfactorily, thus:

Force of A $=$ H d; force of B $= h\,e$; force of C $= h' \{d+e\}$; the same tube being used.

But H $d + h\,e = h' \{d+e\}$; therefore $h' = \frac{\mathrm{H}\,d + h\,e}{d+e}$, as before.

In any solution or compound *d* and *e* being known; and H and *h*, or H and *h'*, or *h* and *h'* being found by experiment, the force may be found, when the tubes are equal, by formula (*a*) or (*b*); or in unequal tubes by (*c*), or (*c*) transposed, if H or *h* be required: and the force of attraction between the solid and the compound, or the solid and one of the component parts of the liquid at any temperature is found by (*d*).

The following table exhibits a few results, the data of the annexed

annexed calculations: I take the altitude of water as the standard:

Name of the Liquid.	Altitude.
1. Water	100
2. Sat. sol. muriate of ammonia	102·7
3. ——— sulphate of potash	95·7
4. ——— sulphuret of potash	95·2
6. ——— muriate of soda	88·2
7. ——— sulphate of copper	84·0
8. Nitric acid	75·0
9. Muriatic acid	70·1
5. Oil of tartar per deliq.	88·4
10. Essential oil of lemon	42·8
11. Alcohol	40·8
12. Refined whale oil	37·5
13. Oil of lavender	37·5

Oil of turpentine, Oil of olives, and Sulphuric æther, nearly the same with 11, 12, 13.

This table exhibits the relative altitudes to which the liquids are elevated; and since it was shown in the former paper, that a change in the density produced by variation of temperature, does not affect the height of the column, these numbers represent the ratios of the forces of attraction between the glass and one particle of the liquid, at the distance to which they are kept asunder by the repulsive force of caloric: and if the force of attraction of the liquid to the solid be required at any temperature, multiply the altitude by the density of the liquid at that temperature.

The following are some effects of combination:

Exp. 1. Saturated solution of subcarbonate of potash, was elevated 23 tenths of an inch . . . . $= h$

Water in the same tube 26·25 tenths . . . . . . $= \mathrm{H}$

Mixture of one volume water, and one volume solution 24·25 or 24·5 tenths . . . . $= h'$

$d = 1$; $c = 1{\cdot}5$.

By formula (a), $h' = 24{\cdot}3$; which is between the two values of h', found by experiment.

Exp. 2. Solution of sulphate of potash (sp. gr. 1·0328) was elevated 89 divisions of the scale.

Altitude of water $= \mathrm{H} = 93$ $\quad d = 54{\cdot}8$

$h = 89$ $\quad c = 56{\cdot}6$

By formula (a) $\quad h' = 90{\cdot}9$.

Exp. 3. Solution of muriate of soda . . . . $h = 22{\cdot}5$

Water . . . . $\mathrm{H} = 25{\cdot}5$

Mixture of equal volumes . . . . $h' = 23{\cdot}5$.

The formula accords closely with the experiment.

Exp.

Exp. 4. Solution of sulphate of copper $h = 95$ $d = 54$
water $H = 113$ $c = 58$
1 vol. of solution + 1 vol. water $h' = 108$
By the formula, $h' = 103{\cdot}6$.

Exp. 5. Alcohol . . . . . $h = 7{\cdot}$ $d = 100$
Water . . . . . . $H = 22{\cdot}$ $c = 83$
1 vol. alcohol + 1 water $h' = 7{\cdot}8$.

By formula (*b*), $h = -9{\cdot}3$; but by experiment $h = 7$.
By formula (*a*), $h' = 15{\cdot}2$; by experiment $h' = 7{\cdot}8$.

Hence the altitude to which any liquid, mixture or combination of liquids is raised, does not depend solely upon the density or densities of the substances concerned: this is evident from Exp. 5; in which, 100 by weight of water, added to 83 of alcohol, causes an addition of $\frac{8}{70}$th only to the column of pure alcohol; although the height of the column of water: that of alcohol : : 100 : 40·8. So extraordinary a phænomenon cannot result from any mechanical law depending upon the relative weight or specific gravity of the component parts; it is more analogous to chemical attraction, and capillary phænomena will afford a measure of corpuscular forces, as well as of their increase and decrease, so far as depends upon distance.

The phænomena of alcohol present apparent anomalies; but they are so constant, that a thorough investigation will develop some of the primary laws of corpuscular forces: the appearances may be partially accounted for by considering that alcohol consists of the elements of 51·15 olefiant (bicarburetted hydrogen) gas, and 31·85 water; the former of which must be repulsive to glass, since a small quantity of alcohol greatly depresses a column of water: thus, 100 water + 83 alcohol rose to 6·66 elev.; 1 vol. of this mixture + 3 water rose to 10·5; 1 vol. mixture + 7 water was elevated 13; water alone 16: in this experiment (since 100 alcohol consists of 61·63 olefiant gas + 38·37 water), 100 of the mixture, proof strength, contains 27·95 olefiant gas + 72·05 water; its altitude was 6·66; pure water 16; 27·95 olefiant gas + 372·05 water rose to 10·5; 27·95 olefiant gas + 772·05 water rose but to 13; *i.e.* if to 100 parts by weight of water, 3·62 of liquid bicarburetted hydrogen gas, as it exists in alcohol, be added, the column is less by $\frac{3}{16}$th than that of pure water.

If the altitude h, to which the liquid olefiant gas alone will rise, be calculated, we find by the experiment on alcohol, that it is $-2{\cdot}2$; whilst, by that on the mixture of equal volumes of alcohol and water, it is about -29.

Whether these curious results arise from an actual force of repulsion between the glass and the olefiant gas, or whether it arises simply from the cohesive force of the upper strata of the

the water (on which the effect depends) being diminished by the mixture of the alcohol, is not apparent.

Another singular example of the agency of heat in effecting a diminution of corpuscular attraction presents itself in the following experiment:—Cut two pieces of soft lead, so that each may have a plain and bright surface; by pressure these surfaces may be made to cohere with considerable force. Suspend the pieces one perpendicularly over the other, and to the lower piece hang weights, nearly as heavy as the cohesive force may be supposed capable of supporting. The application of a degree of heat, not superior to that of boiling water, will cause a separation, provided the weights be sufficiently heavy: whence the corpuscular force of heat produces sensible effects at minute, even sensible distances.

The phænomena of the capillary action of parallel metallic plates are curious. I have made a considerable number of experiments; but until some difficulties shall be surmounted, they cannot be in a state to be submitted to public scrutiny.

An apology is due to you as well as to your readers, for the delay—after I had promised to continue the subject in your last Number: however, I am certain it will not be required, when it is considered that the manufacture of all my own apparatus, which the small resources of a country curacy make requisite, often requires ten times the time which the experimental researches occupy. In addition, the discovery that the column is suspended by the upper stratum only, rendered the experiments of several weeks quite useless, and demands the application of apparatus of a new construction, which is now nearly complete.

[To be continued.]

LXV. *On the Velocity of Sound. By* W. Galbraith, *Esq. M.A.*

To Richard Taylor, Esq.

Sir,

In the 68th volume of the Philosophical Magazine, page 214, I gave a short paper On the velocity of sound transmitted through the atmosphere. In it I endeavoured to investigate an accurate and commodious formula for determining the velocity of sound under given circumstances, embracing all those minutiæ affecting it, so far as I was acquainted with them. In a note, (pages 215 and 216,) I mentioned the values of the constant generally introduced in the late investigations of this question, and hinted that a mean of the whole of these, namely 1·4112, was more conformable to the velocity of sound by experiment,

periment, than that (1·362) which I had usually supposed the more accurate. Now if this be introduced into formula (4), page 217, and the proper value for g also, a slight modification will be effected in the general formulæ for the velocity in metres or English feet.

$$V = (105{\cdot}9518 + 0{\cdot}19845\, t)\left(1 + \frac{f}{5\frac{1}{3}p - 2f}\right)(3{\cdot}14143 - 0{\cdot}0042 \cos 2\lambda) + \omega \cos \phi \; \ldots \ldots \ldots \; (A)$$

the velocity in metres using the metrical barometer and centigrade thermometer.

$$V = \{105{\cdot}9518 + 0{\cdot}1103\,(t - 32^\circ)\}\left(1 + \frac{f}{5\frac{1}{3}p - 2f}\right)(10{\cdot}2739 - 0{\cdot}01378 \cos 2\lambda) + \omega \cos \varphi \; \ldots \ldots \ldots \; (B)$$

the velocity in English feet, employing the English barometer and Fahrenheit's thermometer.

As I have been able to find no experiments by which a direct comparison with these formulæ can be made, except those of Professor Moll of Utrecht, with the omission of the *last term*, namely, $\omega \cos \phi$, I cannot say whether it is in this last respect perfectly correct. As Dr. Moll took the precaution of firing guns at each end of the measured base, the effect of the wind was in this case obviated; and if my formula agree nearly with his experiments, independent of this term, it may be looked on in this state as verified by direct experiment.

The truth of these remarks will be obvious, on consulting Dr. Moll's paper in the Philosophical Transactions of the Royal Society of London for the year 1824, pages 425, &c. in which a full account of the whole steps of his experiments is recorded. At page 445 there is presented a table of the velocity of sound from 44 different experiments on a base near Utrecht in Holland, of about nine miles in length. On the 27th of June 1823, twenty-two shots were fired at each station or end of the base of 17669·28 metres, or 9664·7014 fathoms, the metre being supposed 39·3824 English inches. The sum of the times was $2286^s{\cdot}07$, which divided by 44 gives $51^s{\cdot}96$ for the mean of the whole, which Dr. Moll adopts. Whence $\frac{17669{\cdot}28}{51{\cdot}96} = 340{\cdot}06$ metres, the experimental velocity in one second of time. Now while these 22 shots were fired at each end of the measured base, the mean temperature at both ends of it was $11^\circ{\cdot}16 = t$ of the Centigrade scale. Also the mean height of the metrical barometer was $\cdot 74475 = p$; the mean tension of aqueous vapour by Daniell's hygrometer was $0{\cdot}00925307$ metre $= f$. By substituting these quantities in formula (A) neglecting the last term, $\omega \cos \varphi$, since by firing guns at both ends of the measured base its effect, as Dr.

Dr. Moll observes, page 425, was " annihilated,"—we have,

$$(105{\cdot}9518+2{\cdot}2147)\left(1+\frac{0{\cdot}0092531}{3{\cdot}972-0{\cdot}0185}\right)(3{\cdot}13143+0{\cdot}00105)=$$

$$108{\cdot}1665 \times 1{\cdot}0023 \times 3{\cdot}13248 = 339{\cdot}622 \text{ metres.}$$

Experiment gives . . . . 340·06

Error of formula . . . . −0·438 metre.

In like manner if we compare 14 shots at each end of the base, or 28 at both, fired on the 28th of June 1823, we have,

$$108{\cdot}17742 \times 1{\cdot}00212 \times 3{\cdot}13248 = 339{\cdot}582 \text{ metres.}$$

Experiment gives . . . . 339·34

Error of formula . . . . +0·242 metre.

Whence, these errors being of different signs, we may conclude that it is probable the formula agrees very well with Dr. Moll's experiments, as the mean error of the whole would only be about − 0·196 of a metre, or about eight English inches; a degree of coincidence, in such researches, not to be expected.

Though Dr. Moll has not stated the effect of the wind, yet it may, I think, be inferred from his observations made at each end of the base alternately, as on the 25th and 26th of June 1823.

On the first day, the velocity was 337·39 metres.

On the second . . . . 346·59

Difference . . . . 9·20 metres,

or about 30 English feet.

The same conclusion may be drawn from his experiments when the guns were fired, and heard at both stations.

Thus on the 27th of June 1823, Phil. Trans. 1824, p. 445,

The mean of the one column II. is $\frac{1162^{s}{\cdot}37}{22}$ = . . $52^{s}{\cdot}835$

The mean of column III. is . . . $\frac{1123^{s}{\cdot}07}{22}$ = . . $51^{s}{\cdot}049$

Difference in time is . . . . 1 ·786

Now, $\frac{17669^{m}{\cdot}28}{52^{s}{\cdot}835} = 334{\cdot}424$ metres,

And, $\frac{17669^{m}{\cdot}28}{51^{s}{\cdot}049} = 346{\cdot}124$

Difference . . = 11·700 metres, or about 38 English feet per second, according as the velocity is determined from the one end of the base, or the other.

Now for want of other evidence, we may reasonably suppose that this is occasioned by the effect of the wind accelerating the sound in the one case, and retarding it in the other. Direct experiments are, however, still wanting to settle this point in an unobjectionable manner, though from the state to which

which the investigation is now brought, we may shortly expect the most decisive proof of its just effect. I am, Sir, &c.

Edinburgh, Jan. 21, 1827. WM. GALBRAITH.

P.S. I have since found that the coefficients to the formula may vary on account of the different states of the atmosphere. The quantity by which the formula of Newton before extracting the root should be multiplied, may vary from 1·3 to 1·5, making that adopted lately by M. Laplace, or 1·4, the mean. Therefore 1·3 must be the quantity in very *dry* air, 1·4 in *moist*, and 1·5 in very *damp*.

Hence my coefficients should be

Dry . . .	103	instead of	104·0885,
Moist . .	106	or even	105·9518.
Damp . .	109		

I have come to these conclusions in the mean time, but shall return to the subject at some convenient opportunity. W. G.

LXVI. *Results of the Meteorological Observations made at Wick in the northernmost part of Scotland, published in the Philosophical Magazine.* By W. BURNEY, *LL.D.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

YOU have inserted in your Magazine and Annals, two articles containing Meteorological Observations made at Wick, in the county of Caithness, in the years 1823 and 1825; and as no observations of this kind have been sent to you before from that remote part of Scotland, I have thought their results would be acceptable to those of your readers who are interested in meteorology, by the way of comparison; and have therefore made up their results in two concise tables, with occasional remarks. Seeing some discrepancies in the results of the first article, and knowing that you are very correct in printing, I was induced to go over the calculations for accurate results of the monthly tables; the differences, however, are not considerable. I have also added a scale of the prevailing winds; and to the mean monthly temperatures at 10 A.M. and 10 P.M., I have applied *corrections*, which are the *differences* between the monthly mean temperatures at 10 A.M. and 10 P.M. at Kinfauns Castle, North Britain; and the monthly mean temperatures by a Six's thermometer at that place for 1823, being the nearest to Wick, where a register of the weather was kept at the same hours of observation. The application of these differences as corrections to the monthly mean temperatures at Wick at 10 A.M. and 10 P.M., ought to make the averages nearly as correct as if a Six's thermometer had been used there for that purpose.

Results of a Meteorological Register kept at Wick in the North of Scotland in the Year 1823, and inserted in the Number of the Philosophical Magazine and Journal, for December 1824.

Months. 1823.	Barom. at Noon.	Thermo. at 10 A.M.	Thermo. at 10 P.M.	Mean Temp. at 10 & 10.	Correction.	Approximate Mean Temp.	A Scale of the prevailing Winds. N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
	In.	°	°	°	°	°									
January .	29·72	35·87	35·82	35·845	− 0·049	= 35·796	4	1	1	14	6	...	...	5	31
February	29·25	35·64	33·91	34·775	− 0·071	= 34·704	7	4	2	...	7	4	3	1	28
March . .	29·47	41·28	37·71	39·495	+ 0·866	= 40·361	4	...	...	5	9	8	1	4	31
April . . .	29·68	45·07	40·59	42·830	+ 0·285	= 43·115	12	1	...	...	7	2	2	6	30
May . . .	29·66	50·46	46·04	48·250	+ 1·115	= 49·365	2	2	6	4	5	4	5	3	31
June . . .	29·73	54·03	48·07	51·050	+ 1·018	= 52·068	14	...	...	...	9	2	4	1	30
July . . .	29·52	57·44	51·73	54·585	+ 1·395	= 55·980	10	...	...	3	11	5	...	2	31
August . .	29·59	58·15	51·33	54·740	+ 0·737	= 55·477	1	1	...	4	8	9	5	3	31
September	29·61	53·70	48·98	51·340	+ 0·969	= 52·309	4	...	...	6	1	7	10	2	30
October .	29·52	48·16	45·60	46·880	+ 0·485	= 47·365	3	3	3	1	9	3	2	7	31
November	29·77	46·38	46·22	46·300	+ 0·303	= 46·603	1	...	3	1	8	8	6	3	30
December	29·26	38·18	38·77	38·475	+ 0·194	= 38·669	2	1	2	2	2	13	7	2	31
Averages:	29·565	47·03	43·73	45·380	+ 0·604	= 45·984	64	13	17	40	82	65	45	39	365

Numerical Results for the Year.

		Inches.
Barometer	Max. Nov. 9th & 10th—Wind E. & S.	30·4
	Min. Dec. 1st & 18th—Wind S.W. & W.	28·3
Range of the quicksilver		2·1
Annual mean pressure		29·565
Thermometer	Maximum, Aug. 11th—Wind S.W.	65°·50
	Minimum, Feb. 5th—Wind N.	12 ·33
Annual mean temperature by approximation		45 ·984

The barometrical observations having been registered only to the first decimal place of an inch, there is no doubt a considerable loss in the resulting mean pressure: this supposition is verified by the annual mean result being so low, compared with those of barometers that were placed much higher; as it is $\frac{68}{1000}$ or $\frac{17}{250}$ of an inch lower than that at Kinfauns Castle, where the barometer is stated to have been 129 feet above the level of the sea, and that at Wick only 45 feet, if it were placed at the same height as the thermometer. Considering the difference of latitude of these places, it is curious that the mean temperature at Wick at 10 A.M. and 10 P.M. should be higher in January, February, March, October, November, and December 1823, than at Kinfauns Castle, at the same hours, and lower in the other six months; and also that the annual mean temperatures of these places should coincide within $\frac{111}{1000}$ of a degree: but this must have arisen from the contiguity of Wick to a more open sea, which tends to lessen the chill of the atmosphere in the winter months.

Wind. The prevailing wind, according to the scale in the table, was decidedly from the S. and S.W. points of the compass, in which direction they are influenced by the Western Ocean, and its bluff eastern shore. The wind from the N. was the next in duration, as coming from the open sea. From the S.E., W. and N.W. they were nearly equal, but least from the N.E. and E., from which points they had to travel over the continents of Europe and Asia. Hence it appears that the S.W. wind does not retain its prevailing character at Wick, as it does along the southern shores of England. The prevailing South wind at Wick may be considered as a land breeze, and is a fortunate one for vegetation there. The winds from the S.W., W. and N. are sea breezes; the succulent state of the two first, soon counteracts the sterile effects of the last.

The second article is inserted in the Philosophical Magazine and Journal, for October 1826, p. 317, and contains a table of thermometrical observations made at Wick, at half-past 7 A.M. and half-past 8 P.M. throughout the year 1825. As no results are given in that table, I have therefore calculated the

the monthly temperatures; and the following are their resulting averages, with *corrections :*

Months 1825.	Morning.	Evening.	Means of Morning and Evening.		Cor-rection.		Approximate Mean Temp.
	°	°	°		°		°
January	39·18	39·93	39·55	+	1·79	=	41·34
February	38·19.	39·37	38·78	+	2·37	=	41·15
March. .	40·42	41·64	41·03	+	2·48	=	43·51
April . .	43·28	43·53	43·41	+	2·55	=	45·96
May . . .	47·28	47·13	47·20	+	2·06	=	49·26
June . . .	52·88	53·24	53·06	+	1·09	=	54·15
July . . .	57·48	56·65	57·07	−	0·10	=	56·97
August .	55·91	55·02	55·46	+	1·91	=	57·37
September	54·42	53·86	54·14	+	2·40	=	56·54
October .	47·44	47·31	47·38	+	1·89	=	49·27
November	37·09	37·85	37·47	+	1·60	=	39·07
December	39·59	38·78	39·18	+	2·08	=	41·26
Averages:	46·096	46·192	46·144	+	1·843	=	47·987

The above corrections are the *differences* of the mean monthly temperatures by a Fahrenheit's thermometer at Gosport, taken nearly at the same time in the mornings and evenings as at Wick, and the monthly means by a self-registering thermometer; and are applied to the average temperatures of the morning and evening at Wick, in order to obtain an approximation to the real mean temperature of that place. But as these corrections were obtained from simultaneous observations here, perhaps they have made the approximate mean temperature of Wick, nearly half a degree too high. From these results, which were made to compare the temperature of the atmesphere at the remote part of Scotland with that at the southern parts of England, it appears that the difference in the approximate mean temperature at Wick, and the real mean temperature at Gosport, Hants, in 1823, was only 4½ degrees, and in 1825, *five* degrees, notwithstanding the former place has a greater north latitude than the latter by 7° 14′.

I am yours, &c.

Gosport, March 5, 1827. WM. BURNEY.

LXVII. *Re-*

LXVII. *Remarks on* Mr. Sturgeon's *Paper "On the Inflammation of Gunpowder by Electricity."* *By* Mr. Thos. Howldy.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN consequence of some passages in Mr. Sturgeon's paper, inserted in your Number for January, appearing to me to insinuate that my experiments on the inflammation of gunpowder were not *actually* performed in the manner which I stated, but that I *concealed*, either designedly or ignorantly, some *essential particular* which rendered them successful, I feel the necessity of requesting the favour of your inserting my present letter. Mr. Sturgeon introduces the subject of my experiments with the following observation. "The most extraordinary method of effecting the ignition of gunpowder by the electric fluid, that I have yet heard of, is that stated by Mr. Howldy, in the Philosophical Magazine, vol. lxviii. p. 173." This is a strong, though an indirect, testimony that my method is at least *original.* He then proceeds: "I have been induced," says Mr. S. "to pay some attention to this method."—"I think, however, it is to be regretted that Mr. Howldy has not mentioned the hygrometrical state of that part of the table ('four inches') between the extremity of the chain and the outside of the jar: as it is possible that a variation in that particular may vary the result of the experiment." Now, I can relieve Mr. Sturgeon's regret, by assuring him that the part of the table which he has mentioned and suspected, was *perfectly dry*, and free from moisture of every kind, as was likewise every other part of the table, and the apparatus employed, when the experiments in question were made. The table is of elm; it was made and has been employed solely for the purpose of supporting electrical and other apparatus. Mr. S. next remarks, "Considering, however, that four inches is a long *striking* distance through dry air,—" It would indeed have been a long striking distance, under the circumstances I have described, supposing the charge to have actually *struck* or passed over that distance in the form of a spark. But the distance or interval was made *long*, expressly for the purpose that the charge *might* NOT strike over it; for if the charge had struck over that interval, the experiment would not have been successful. For in any arrangement that is made according to my method, when the jar is discharged, whether the interval between it and the chain be four, three, or two inches, the only appearance in that interval is a little light at and near to the extremity of the chain; from which place the charge becomes

becomes so attenuated in its passage to the jar, as to be incapable of affecting the organs of sight. This circumstance very frequently occurs when the charge is directed over an imperfect conducting surface, if the distance be greater than that over which the charge can *strike* in the form of a spark, or with explosion.

If Mr. Sturgeon is acquainted with this fact, he has on the present occasion inadvertently employed the expression, *striking distance.*— The electric fluid by expanding or diffusing itself over an imperfect conducting surface of some extent, often passes as invisibly as it does when it pervades a metallic conductor. In the next clause to that which I have been considering, Mr. S. says: "not *happening* to be successful when attempting to repeat the experiment, according to Mr. H.'s directions, I have been induced to suggest to that gentleman the necessity of his repeating the experiment, under the following circumstances."

Now, if Mr. Sturgeon found the experiment to be either impracticable, or to be more difficult to be performed successfully by my method than by that with the water tube, he ought, I submit, for his own credit and the satisfaction of his readers, to have pointed out clearly and demonstratively in what the difficulty or impracticability consisted. The statement of facts, and the observations which I have made relating to the subject in discussion, will relieve me from the necessity of repeating the experiment under the circumstances Mr. S. has suggested, especially as I perfectly agree with him that "there would recur different results." Mr. S. seems to have misunderstood a part of my statement concerning the wooden point or peg, and has asserted that the celebrated electricians Dr. Watson and Mr. Wilson "used it in that shape more than fifty years ago." I must observe, however, that Priestley, in his History of Electricity, (4to, fourth edit. 1775,) has given an account of Dr. Watson's experiments; but he has not stated that Dr. Watson used a wooden point in any of them. And as to Mr. Wilson, I have his own account of the experiments he made, under the patronage of His late Majesty, in the large room at the Pantheon; and I do not find that in any of his various experiments he employed a wooden point: and the manner in which he inflamed gunpowder shall be told in his own words: "Upon a staff of baked wood a stem of *brass* was fixed, which terminated in an *iron point* at the top. This point was put into the end of a small tube of Indian paper, made somewhat in the form of a cartridge, about one inch and a quarter long, and about two-tenths of an inch in diameter. When this cartridge was filled with common gunpowder

powder (unbruised), the wire of communication with the well was then fastened to the bottom of the brass stem. Being so circumstanced, and whilst the charge in the great cylinder and wire was continually kept up by the motion of the wheel, the top of the cartridge was brought so near to the drums as frequently to touch the metal. In this situation a small faint luminous stream was observed between the top of the cartridge and the metal drum. Sometimes this stream would set fire to the gunpowder at the instant of the application; at others, it would require half a minute, or more, before it took effect." p. 75.

I must now beg to call upon Mr. Sturgeon to verify his assertion, by giving a reference to any electrical experiment which either Dr. Watson or Mr. Wilson made with a wooden point. I am, Gentlemen, your obliged servant,

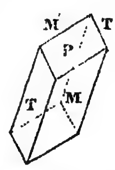
Hereford, Jan. 11, 1827. THOMAS HOWLDY.

LXVIII. *On Chromate of Silver. By Mr.* E. F. TESCHEMACHER.

To Richard Phillips, Esq.

Dear Sir,

I BEG to hand you herewith a small quantity of crystallized chromate of silver, obtained by allowing a solution of chromate of potash (after separation of the precipitate occasioned by nitrate of silver) to evaporate spontaneously: at the end of ten days these crystals were deposited at the bottom of the vessel. The crystals have a strong metallic lustre, and are of a deep red colour by transmitted light, much resembling native red silver. They are insoluble either in cold or hot water. The primitive form appears to be a doubly oblique prism of the following measurements, taken by Dr. Wollaston's reflective goniometer from very brilliant natural planes.



P on T . . . 123°
P on M . . 101° 05′
M on T . . . 69° 55′, or on T′ 110° 05′

On platina wire before the blowpipe it gives a deep emerald green glass. On charcoal the silver is reduced, appearing in small globules on the surface of the chromic oxide. That this substance is a bichromate of silver, I have proved by directly combining the chromate with an additional portion of chromic acid.

I am, dear sir, yours, &c.

Barnsbury, March 13, 1827. E. F. TESCHEMACHER.

LXIX. *On the Geology of East Norfolk: with Remarks upon the Hypothesis of Mr.* Robberds, *respecting the former Level of the German Ocean. By* R. C. Taylor, *Esq. F.G.S.*

[Continued from page 290.]

IN a preceding paragraph of this paper a proof of the violent disruption of the strata is exhibited in the enormous detached masses of chalk, lodged in the diluvial cliffs and occupying a position *above* the crag, which is seen distinctly stratified, reposing upon the plane of the original or main body of chalk.

An examination into the materials of this range of cliffs will introduce to our notice more ancient formations, whose traces are not here so readily accounted for. The shore to the west of Cromer exhibits a singular accumulation of travelled fragments of primitive rocks, whence it would not be difficult to collect a tolerably illustrative series. They consist chiefly of rounded blocks of several varieties of granite, basalt, porphyry, trap, and micaceous schist; sandstones of various kinds, chert, breccia; besides limestone, claystone, &c. I am not sufficiently acquainted with the nomenclature of rocks to hazard a more detailed enumeration here. The diluvial fragments of the later series are from the chalk, the plastic clay, London clay, green sand, Kelloway's rock, the oolites, lias clay, and marlestone;—in fact, almost every formation above the coal measures. These are of all intermediate magnitudes, up to four tons weight. Large bouldered masses may be seen in the sea at low water, lying mixed with flints, upon the chalk. One block of granite was observed near six feet in diameter. Another mass standing six or eight feet high, has for years been known to the fishermen under the name of Black Meg. This collection extends about two miles, chiefly opposite to Beeston Hills. At Happisburgh Cliff the blue diluvial mud or clay appears to contain rolled fragments of various primeval and secondary rocks. Among them have been observed granite, basalt, sandstones, black siliceous pebbles, septaria, and micaceous schist containing small garnets.

Whence has this singular assemblage been derived? Certainly not from ballast; the size of many of these stones prevent that supposition, as does the locality of their position. Had they drifted from our northern continental shores, as has been suggested, one would expect to find them dispersed generally along this coast. But the question appears decided by the fact that they are chiefly, if not entirely, supplied by the diluvial

diluvial clay in which they may frequently be noticed, whence I have collected many specimens, and others may be seen several miles inland. It is probable that these large masses, after being dislodged from the cliffs, are very little removed from their original sites by the action of existing currents in the ocean. This opinion is confirmed on considering the other circumstances attendant on the wearing away of these cliffs. As might be expected, the alluvial substances that form the beach and line the shores of the eastern counties, are, for the most part, obviously derived from the small rounded fragments, and the ancient water-worn gravel, which the sea has washed from its precipitous borders. But there remain extensive portions which do not appear to have changed their position, further than would be occasioned by the dissolution of their matrices. These instances occur, with few interruptions, in a course of 12 miles, from Mundesley to Salthouse Bay, and in particular at Foulness. A series of irregular ridges of unrolled angular flints, locally termed *rocks*, mark the ancient sites of denuded chalk beds, and prove valuable barriers by checking the violence of the surf in northerly gales. No change in the forms of these ridges of chalk flints is perceptible. The ocean, mighty and furious as it sometimes is, will partially overspread them with sand, but is incapable of shifting the heavy interlocking materials which offer the only permanent obstacle to its encroachments.

As regards the boulders of miscellaneous rocks, it may be observed, in concluding that portion of our subject, that they occur chiefly upon the beach and sloping shores; not on those external ridges over whose sharp irregular surfaces they can scarcely be imagined to have rolled.

Whilst reviewing the geological phænomena attendant on the eastern coast and valleys, we must not lose sight of those which are observable in their upper extremities and ramifications. These are lined, not with an oozy sediment as in the æstuaries, but with moor and forest peat, to the depth of six or seven feet. In the operations of cutting drains and turf, the horns of large ruminant animals have been discovered; and in the gravelly margins occur vertebræ and teeth of elephants. There is a considerable deposit of peat in that valley in which the Waveney and Little Ouse have their sources; and trunks of trees, whose wood is yet hard, are sometimes taken out and used as fuel. Horns of deer are occasionally met with in the fens of Lopham, Hinderclay, Redgrave and Bressingham. It is an interesting fact also, that in the peaty valley of the Waveney, at Roydon, Diss and Hoxne, have been found ancient flint axes. Teeth and large bones prevail in the diluvial gravel of the

same valley at Hoxne and also at Eye. Vertebræ occur in another branch near Botesdale, and in some valleys on the eastern coast of Suffolk.

The gravel of the Bure valley has furnished many animal remains, as also have the banks of the Wensum. Mr. Parkinson long ago observed that this district supplied a greater abundance of fossil bones of deer than any other part of this kingdom.

But the most considerable discovery was very recently made, in a valley near North Walsham, in the process of digging an extension of the Dilham Canal. These specimens have, by permission of the Canal Company, been deposited in the Norwich Museum. They consist of horns of two species of deer, and skulls apparently of the fossil auroch or bison, and of the common ox.

It will be observed, that the localities which are here recited, are all within the limits which may be assigned to the Crag; and it must be added that in frequent instances they are accompanied by decided traces of that formation. Although they are comparatively remote from the sea, their sites are not more than 40 to 80 feet above its level. The ancient stone axes and other implements found in the peat of the Waveney valley, denote its alluvial covering *. Some of the bones

* The Waveney valley has exhibited traces of the early occupants of this district in greater abundance, and more generally distributed, than any other portion of East Anglia. From Garianonum even to Thetford (the ancient Sitomagus), coins, medals, urns, and other reliques of Roman origin, have been found at numerous points; indeed in almost every parish bordering upon this valley,—indicating it to be a favourite position with that people. Many local circumstances concur to render the occupation of this valley desirable to the adventurers of various nations, who from time to time penetrated into the interior, through one or other of the channels of the Garienis. At Thetford, and at several points near the upper part of the Waveney, Celtic and Scandinavian remains of military weapons have been discovered. Flint axes and copper celts have been found in the neighbouring parishes of Roydon, Diss, Scole, and Hoxne. The stone axes appear to be similar in form to those to which the original Teutonic appellation of *Staimborts* or *Steinbartes* is given by Dr. Hibbert, occurring in Orkney and Shetland.

They have also been met with in the vicinity of the Humber, near the confluence of the Trent and the Ouse, being probably brought hither by the Saxon and Scandinavian pirates that from the earliest period infested the shores of this country. A comparison of the forms of these ancient weapons, the substances of which they are constructed, and the circumstances under which they are discovered, aided by the scanty historical materials hitherto collected, is necessary to determine their origin with any degree of precision. This is a task which, I believe, antiquaries have not hitherto attempted. The copper instruments of war are observed chiefly in those districts which were occupied by Celtic tribes. As regards the stone axes, the authority alluded to, states, that in whatever country of Europe weapons

bones probably belong to animals who fed and died in those situations; while others appertain to species that were extinguished by the last catastrophe which affected our earth.

In two other moory valleys within the diluvial district of Norfolk the horns of stags have been found: the one at Carbrooke, near the head of the Stoke river; the other at East Bilney by Dereham, communicating with the Wensum valley.

From all that has been said in the foregoing pages, it will appear that the fossil bones occupy no specific place in the upper marine formation. They are found equally lodged in the higher valleys as at the lowest point to which the sea retires, and even on shoals some miles from the shore; and though frequently unaccompanied by crag shells, are most commonly blended with them. Sometimes they rest, in good preservation, in ancient peaty depositions, or lie, as if thrown by currents, amongst heaps of marine shells; and in other cases, broken and partially rounded, they are imbedded in diluvial gravel.

We can therefore only conclude that the existence of those races was equally contemporaneous with the crag and with the buried sylvan tract at the base of the cliffs, and that it

pons similar to them might be found, a visitation from the Vikingr, or Sea-kings of the North, is strongly indicated.

Having obtained certain data, approximating to chronological accuracy, we shall thence be enabled to observe, with some precision, the time occupied in forming alluvial depositions, since those early traces of man, those rude works of art, were deposited in our valleys and morasses. The canoes, the implements of war and of commerce, the works and the personal ornaments, both of the aboriginal inhabitants and their successive invaders, are occasionally exposed or raised from beneath extensive peat formations in this island. They are, indeed, the only criteria by which to mark and measure the extent of this alluvial process in given periods, and in particular situations, during the succession of ages which have elapsed since the surface of our soil was habitable to man. From the many instances which have occurred of these geological chronometers, two only will be selected as bearing more immediately upon the main topic of this paper, and but little removed from the district under consideration. In the marshes of the Medway, which nearly resemble those of East Norfolk, several canoes were dug up, in 1720, in all respects similar to those which are ascribed to the ancient Britons; being composed each of a single tree, hollowed by fire, precisely in the same manner as those of the North American Indians.

The other instance is the Roman Causeway, supposed to have been made by the Emperor Severus, extending from Denver to Peterborough, across the Fens of Cambridgeshire. It was composed of gravel three feet deep and sixty feet broad, but is now covered with moor or peat from three to five feet in thickness. In both these cases, particularly in the latter, evidence is produced of an *increased elevation of surface*, and the gradual formation of solid land, either by the deposition of oozy sediment, or by the growth and decay of vegetable substances; and data are supplied, as in the case of the ancient anchors in the Garienis, for measuring the extent and duration of that process.

ceased at the epoch which invested the whole with their diluvial covering.

That the crag shells and their accompaniments form a comparatively local deposit, like some other formations, is evident. Its existence and its peculiarities, nevertheless, indicate a distinct geological æra.

Let us recapitulate the remarkable facts, by which, in this case, the questions of identity and general continuity are determined.

A district, bordering a hundred miles upon our eastern coast, is occupied by an ancient marine deposit, continually changing its aspect, yet constant in its peculiar characters, and always to be understood by unerring data: now appearing as a ferruginous sandstone, then in compact clay, and again considerably indurated; sometimes blended in a mass of extinct zoophites, sponges and alcyonites, forming a soft rock; oftener an irregularly accumulated mass of decomposed and broken littoral shells, loosely imbedded in sand like an ordinary sea beach, yet accompanied with the remains of unknown animals. Sometimes forming the substratum of a considerable area; or, overwhelmed beneath the debris of older strata, only detected at intervals. At one point exhibiting groups of shell-fish allied to those of the neighbouring sea, and at another composed of numerous genera, which are neither to be recognized living in any part of our globe, nor assimilating to the fossil shells of other formations. Can it be doubted then, that we must look to an earlier epoch in the geological history of our earth; to a period prior to that in which our diluvial eastern counties received their existing shape and covering; and to other, but not less extraordinary operations upon their antediluvian surfaces, than those to which Mr. Robberds's views would limit us, in accounting for the phænomena to which he invites our attention, and of which I cannot but consider an erroneous application has been made?

I have been led to introduce this sketch of the most remarkable deposits above the chalk, because they have been hitherto neglected; and because it was desirable to elucidate, with precision, the geology of the limited district to which these observations are directed,—so far, at least, as it bears upon the assistance which the phænomena it exhibits have afforded to the peculiar views of Mr. Robberds. At this stage of the inquiry, conclusions incompatible with those views are unavoidably suggested by a consideration of the data here supplied.

Enough has been advanced, to withdraw from inferences, apparently so unobjectionable, the very basis upon which they are

are founded. May I not here be allowed to register those premises and those conclusions which, after a careful oryctological examination, appear opposed to those of Mr. Robberds?

The shells which are deposited at the height of 40 feet, on the sides of certain valleys, belong to an antediluvian formation; *therefore* they cannot be admitted as evidence of supposed changes, or of events that have occurred subsequently to the deluge.

There is no direct or reasonably inferred proof remaining of the postdiluvian operations of the sea at such an elevation upon our coasts; *therefore* the assumption that the German Ocean was 40 feet higher than at present, and has gradually fallen, is unsupported.

That at an early period of what may be termed, in geological phrase, the existing state of our globe, the sea entered the mouths of these æstuaries, and rolled its tides far up into the interior, I can no more doubt than the respectable authority, who has collected so many indisputable proofs. The difference, and that no trivial one, between us, lies in the amount. I differ as regards the quantity and elevation of the tidal waters, *after their admission into these valleys*; being satisfied that such elevation was inconsiderable, and that in no sensible respect were the waters of the surrounding ocean, since the existence of man upon this island, higher than at the present moment.

To render this more intelligible, it is necessary to trace the causes of the change in the level of the inland waters, and of the bed of the valleys themselves, to their probable origin; commencing from the period at which it is proved that such flats openly communicated with the sea.

The set of the great tidal current of the German Ocean is from the North-west, along the eastern shores of this island. It consequently happens, that wherever any portion of the land projects beyond the general line of coast, and consists of any material which yields to the action of those tides, such exposed points have, from the earliest recorded periods, been gradually reduced and rounded off, and the debris has been uniformly deposited to the southward; either forming shoals in the sea, or elevating low tracts of land upon its borders. Thus the detritus of the chalk strata at Flamborough Head and the diluvial cliffs of Holderness have contributed, in the slow progress of years, to increase the alluvial districts near the Humber. In their progress southward, the tides next meet with an extensive obstruction in the projecting county of Norfolk. About twenty miles of its coast has been subjected, from time immemorial, to the abrasive action of ocean currents.

The

The ancient villages of Shipden, Wimpwell and Eccles have disappeared; several manors and large portions of neighbouring parishes have, piece after piece, been swallowed up by the encroaching waves; and their site, some fathoms deep, now forms a part of the bed of the German Ocean. Cooperating with the tides, the land springs in the bordering high grounds are constantly, though slowly, working to reduce our boundaries. Enormous masses, dislodged by the pressure of the springs, are continually precipitated upon the beach from the high cliffs, to be carried off by succeeding tides. In the winter of 1825, one of these fallen masses covered twelve acres, extending far into the sea; its upper portion having fallen from a height of 250 feet, near the light-house at Cromer. The effects of this destructive process are traced in the banks and shoals extending 20 miles to the southward, and in the formation of the low flat tract between Happisburgh and Gorleston. In their progress the tidal currents possess sufficient strength and velocity to preserve a deep channel, locally called Roads, parallel with the shore; but they deposit, both on the sea and land sides of this passage, the alluvial matter with which the waters are charged. Mr. Cubit has appropriately denominated this channel, a sea river. A portion only of the substances that form the shoals and sand-banks may be considered shifting, and these are modified by every variation of wind and tide. The nuclei of most of the largest appear to be permanent, and probably existed at a period far more remote than we can estimate. Thus the antiquity of the Holm-sand, opposite Lowestoft, is decided by its Anglo-Saxon name.

There is reason then to believe, that the removal of one part of the Norfolk coast has led to the consolidation of another; and has tended to silt up and raise the bed of the æstuaries to such a degree, as almost to exclude the ingress of the tide. In their present state, they are filled to the depth of many feet with ooze, accompanied with fluviatile shells. Gravelly knolls arise, at intervals, to the surface; and banks of shells, partly drifted from the ocean, and partly consisting of those *tellinæ*, *mactræ*, and other genera usually found associating in oozy beds near the mouths of large rivers, are occasionally discovered. After having passed some miles from the sea, the rivers are contracted by the growth and decay of aquatic plants, forming unembanked margins locally called Rands; and similar recent formations of marshy peat are gradually narrowing the broads. Except under such circumstances, the main æstuaries exhibit no ancient accumulations of peat, no large trunks of trees, or other indications of a

freshwater

freshwater valley. Every thing denotes that their beds were gradually heightened by the deposition of a marine sediment. In this there is nothing remarkable; the operation is daily going on on the Lincolnshire coast, where instances are related of the precipitation of a stratum of mud an inch thick, in a single day. The industry of man enables him to avail himself of this tendency;—the operations of nature are assisted by art, and large tracts of the richest land have been artificially produced from the earthy materials brought by the waters of the ocean. Had we need to travel further than the adjoining county, this article might be extended by reciting remarkable illustrations of the formation of alluvial lands, in the numerous and fertile Danish islands of the Baltic; in the *marsches* on the coast of Sleswick, or in the Deltas on the shores of the Adriatic. A due examination into all the facts by which local changes are accomplished,—whether in the gradual absorption of high lands, on the one hand, or in the progressive emersion of extensive flats, on the other,—in either case the result of existing causes, has always ended in confirming the general principle of the permanent residence of the sea at one level, and without diminution, since the deluge. All the apparent exceptions are referable to local and existing causes; as in the cases of coral reefs, of muddy depositions, or volcanic agency, which affect not the surface of the ocean, but elevate or depress the base upon which its waters repose. The balance of these fluctuations leaves the water level where it ever stood: or if any alteration could be perceptible over so extensive an area, it would be an elevation corresponding to the disintegration of the land.

[To be continued.]

LXX. *Corrections in Vlacq's Tables of Logarithms.*

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

THE importance of having our tables of logarithms accurately printed, makes us greatly indebted to such men as Mr. Babbage, when they will undertake the tedious labour of minutely examining and correcting them. The same reason induces me to submit to you a few observations on the list of corrections, which are enumerated in page 300 of your present volume.

Every one knows the disgraceful carelessness with which

Vlacq's great work was printed at Gouda in 1628: and upon examination I find the following results for the numbers in Mr. Babbage's list.

	Logarithms as printed by Vlacq.	Logarithms as they ought to have been printed.
24626	39139·39751 .	. 39139·38751
38962	·59064·13420 .	. ·59064·12420
57628	76063·35875 .	. 76063·35475
57629	76064·10436 .	. 76064·10836
63747	80445·97412 .	. 80445·97512
67951	83219·58424 .	. 83219·58524

I have placed a mark over the corrected figures: and it will be clear that, if we reserve only seven places of decimals,—and we wish to have the last of them true to the nearest figure,—Mr. B.'s corrections will be necessary for the last five numbers in this list. It should, however, be remarked in justice to Taylor, that he had detected the mistake in the logarithm of 38962, and has noted, in the errata printed on the last page of his tables, that the last figures should be 412, not 413. Indeed the cost of time, labour and attention must have been immense, to have avoided a greater number of errors. Some idea of the difficulty may be formed, when it is seen that even Mr. Babbage, with all his most laudable anxiety for accuracy, has not been able to avoid mistake. It seems to have escaped him that Vlacq's logarithms for 24626 was wrong, and that the last figures of the seven decimals are, therefore, not 940, as he suggests that they ought to be, and as they are in Gardiner and Hutton, but 939, as they are given by Taylor.

Allow me to take this opportunity of pointing out to you that your note at page 271 is incomplete. You should have added that M. Gay-Lussac, who ascended higher with his balloon than MM. Sacharoff and Robertson, found no such variation as they describe, in the intensity of the earth's magnetism. There is an account of his voyage in the 21st volume of the Philosophical Magazine, at page 220.

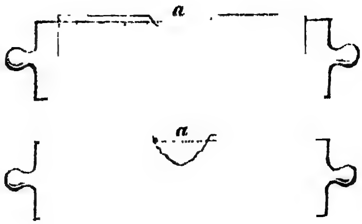
April 12, 1827. N. R. D.

LXXI. *Theory of the Spirit-Level.* *By* J. Nixon, *Esq.*

[Concluded from p. 263.]

IN order to acquire some knowledge of the variation of figure produced in the bubble of a spirit-level in consequence of the reciprocal attraction of the glass tube and the contained

contained liquid, the following experiments were made with a straight glass tube having a bore of 0·5 inch. Both ends being stoppered, an opening *a*, of an irregular figure, about 0·2 in. across and 0·3 in. deep, was made in a part of the tube equidistant from the ends. The tube being placed horizontally with the orifice upwards, was filled with water, which stood within it apparently the same in volume and figure as though the tube had been entire and impervious to the air. Drawing out the stoppers, so as gradually to augment the space between them, the atmospheric air soon made its ingress, causing that portion of the water immediately under the orifice on which it was incumbent to assume the concave surface, of which a section is given in the upper figure. But as the interior space continued to augment, the bubble of air elongated towards the stoppers without material addition to its depth; its ends being equidistant from *a*, and curved precisely as those of the bubble of a spirit-level. On pushing the stoppers *home*, the bubble, repassing through the same changes of figure, was finally expelled from the tube.



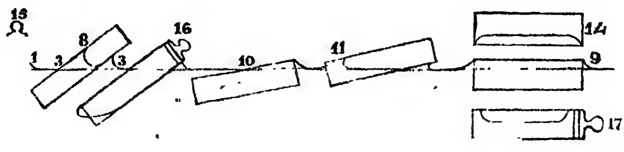
The tube being well dried, the experiment was repeated with mercury, which not only filled the tube, but stood at *a* quite out of it. On increasing the interior space, the mutual repulsion of glass and mercury gave room to the latter to shrink in the first instance from the upper corners of the tube, thus checking the admission of the air until a further addition of space within the tube caused the mercury to subside throughout its length with a surface nearly horizontal, but terminating at the stoppers in a convex curve as before*.

In order to enable us to comprehend, in some degree, why the water filling the horizontal tube, so soon as the containing space became enlarged, should not subside equally the whole length of the tube, and come to rest with a horizontal surface of which the bounding lines (a right-angled parallelogram) would be in every point in contact with the sides of the tube or ends of the stoppers, it will be necessary to notice such of the phænomena of capillary attraction as appear calculated to illustrate the subject.

(1). When a vertical cylinder or plate of glass is partly im-

Had the stoppers been of platinum, what would have been the figure?

mersed in water, a portion of the water rises immediately around them with a curved surface about 0·1 in. above its na-



tural level. (2). If two vertical cylinders or two plates of glass be brought nearly in contact, the water will ascend higher between them than on the opposite or other sides. (3). On inclining a plate or cylinder of glass, the water under the overhanging side of either will rise to a greater perpendicular height than when they are vertical; but on the opposite side the elevation will be so much diminished as to be nearly insensible. (4). When two plates of glass, inclined to each other, are immersed in water with the line of their intersection vertical, the water will ascend between them, and form a hyperbola. (5). One end of a vertical tube having a bore of 0·5 inch being immersed in water, the water immediately surrounding its exterior appeared to stand as much above its natural level as within the tube. (6). But when capillary tubes, that is, those of which the bore did not exceed 0·1 inch, were made use of, the water within the tubes stood with a *concave* surface at a much greater height than on the outside; the discrepancy augmenting nearly as the bore of the tubes diminished. (7). Within a *tube* the ascent of the water, on account of the quantity of attracting zone being nearly double that of two vertical *plates* placed at a distance from each other equal to the diameter of the tube, is about double its elevation between the plates. (8). For the same reason the vertical height of the water within the half-inch tube when inclined, was greater on that interior side of it overhanging the water within, than on the corresponding exterior side (9). On drawing the same tube horizontally out of the water, it continued to be filled exclusively with that fluid, even when its interior upper surface was elevated nearly 0·1 inch above the general level. (10). This was equally the case when either end of the tube was submerged in, and the other stood out of the water not more than 0·1 inch. (11). On raising either end of the tube *more* than this height out of the water, the air began to intrude, impressing on the fluid the curved form exhibited in the figure. (12). The tube being subsequently held

held with the lower end rather more than 0·1 inch above the water, the air entered also at that depressed end, and in effecting a junction with the air previously introduced at the upper orifice, burst a film or thin plane of water extended across the tube almost close to the depressed end. (13). This film (which would abruptly form again, and in the same place on lowering the tube) was ruptured, on taking the tube *horizontally* out of the water, at the middle of the length of the tube. (14). When completely out of the water, a portion of that fluid remained at the bottom of the horizontal tube at a depth of about 0·1 inch. (15). One end of a tube, having a bore capable of raising water half an inch above its level, being hermetically closed, a disk of thin paper covering the other orifice was secured to the tube by means of melted wax. This end (purposely left rather moist within) being placed vertically in water at a considerable depth, the wax and paper were forced completely off; yet the water, from the resistance of the included air, did not rise more then 0·1 inch within the tube. (16.) Repeating the experiment with the half-inch tube, the water stood within it about the preceding height; but in bringing the tube nearly horizontal, the included air protruded a little beyond the upper part of the orifice, continuing to escape gradually in small bubbles. (17.) When the tube, wholly immersed in the water, appeared to be exactly horizontal, the residue of the air, in figure like the bubble of a level, reached within 0·1 inch of each end, and did not attempt to escape until the stoppered end was slightly depressed, when the whole rushed abruptly out. But when the tube, in a subsequent experiment, contained only a small quantity of air, so that the (more spherical) bubble was at a considerable distance from each end, the stoppered end was obliged to be depressed several degrees, before the bubble could complete its escape.

On a careful review of the experiments, will not a diminished specific gravity of the water immediately in contact with the glass, together with the cohesion of its particles, account for every observed violation of hydrostatics? In a narrow tube the particles of water within it may be considered as equally acted upon by the maximum of the force peculiar to the surface of the glass which tends to diminish their specific gravity; whereas the particles within a wide tube are mostly at some distance from that force, especially where it is greatest, and are also influenced in a contrary sense by the cohesion of a greater number of particles beyond the effect of the force in question. Hence the same external pressure causes the lighter water within the small tube to rise higher than the heavier

water

water within the wide one: and for the same reason the water fills the horizontal tube (9), even when held above its general level. No. 16 proves very clearly that a force of attraction perpendicular to the axis of the tube does not elevate the fluid, —or why should the included *elastic* air wholly prevent its wonted ascent? If such a force did exist, why should an almost insensible depression of the horizontal tube in No. 16, overcome it so completely as to expel the bubble of air? A globule of air rises to the surface of a liquid in consequence of the pressure upwards against its base being greater than the downward pressure on its upper surface: hence the greater the diameter of the globule, and the greater the force with which it is urged upwards; which may serve to account for the tardy and impeded escape of the minute bubble on repeating the 16th experiment. Besides, as the small bubble has to move at an almost insensible distance from the *curved* surface of the glass, where the levity of the water must be very great, its specific gravity approaches more to that of the water in contact with its ends, and will therefore require a considerable depression of the tube to make the vertical column urging it below adequately heavier than the opposed one incumbent on it. But when the bubble is long and deep, the pressure upwards is derived from an incomparably longer and specifically heavier vertical column acting against diminished obtacles.

According to our theory, the specific gravity of the liquid within the horizontal tube of a level will be least at the upper corners of the tube; but along the upper surface, and still more so on the sides above the level of its axis, it will approach in a greater degree to its proper specific gravity. Hence the surface of sections of the liquid, whether perpendicular to, or in the direction of the axis, but especially the latter, will have a concave curve at the extremities; which will account for the peculiar figure of the bubble of a spirit-level.

(We may now comprehend how an uncurved tube containing a sufficient quantity of ether, &c. becomes possessed of a bubble which does not extend to the ends of the tube. In a level of this description, as its bubble, when the cylindrical tube is horizontal, will be stationary indifferently in any part of its length, and as the slightest inclination would displace the bubble (if sufficiently large) indefinitely, causing it to move at once to the elevated end of the tube, it could not serve to measure the minutest variation of inclination.)

Let the bubble of a curved tube pass over 0·1 inch for a variation of inclination of 1″; then if its depth in the middle be 0·2 inch, its length, when the tube is horizontal, should be equal

equal to 15 feet. But as the levity of the water near the extremities of the arc is greater than under the vertex, it must stand higher there than in the latter place, or an equilibrium cannot be effected. From this cause the length of the bubble becomes reduced from 15 feet to little more than as many tenths of an inch: however, as the reduction will be the same at each end, provided the tube be a perfect ring, the middle of the bubble will still coincide with the vertex of the arc. For reasons already assigned, it is nevertheless requisite that the bubble should be of a proper length and depth.

When a vertical plane describes any arc of revolution about a horizontal line or axis, a straight line, as the radius or chord of a circle, previously drawn anywhere on that plane, will have its inclination to the horizon varied by an angular quantity equal to that arc of revolution. This will be as obvious for an excentric line as it is for one intersecting the axis, when we consider that their parallelism or angle of inclination to each other remains constant. If we therefore describe with the same radius two circles on the vertical plane, one concentric and the other excentric to its axis, and mark their vertex points before, and also after any partial revolution of the plane, the lineal distance of the two points on the one circle will be exactly equal to that of the corresponding points on the other. Were we to fix anywhere on (but parallel to) either vertical side of our circular vessel* the tube of a spirit-level having an equal radius of curvature, then, as the vessel revolved, its bubble would pass over the same lineal space as that of the tube. Should the radius of curvature of the tube exceed that of the vessel, the arc of revolution (or variation of inclination), as measured by the graduations on the rim of the latter, would nevertheless correspond with the indications of the scale of the level. With this explanation we may now comprehend how a spirit-level, having a radius of curvature of several hundred feet, although fixed (parallel) to a vertical plane (such as that of an astronomical circle) within a few inches of the axis of rotation, should have its bubble displaced by the same (sensibly) lineal space as though that axis coincided with the centre of its circular curve.

To construct an instrument called a *level*, capable of determining the horizontal inclination of straight lines, planes, &c. the curved tube of a spirit-level furnished with a scale is fixed with its convex side upwards to the upper surface of a straight bar of brass, wood, &c. in such a manner that the plane of the circle of curvature of the tube shall be perpendicular to

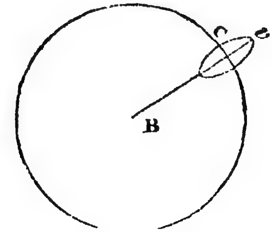
* See above, page 257.

the under surface (a right-angled parallelogram) and parallel to the (longitudinal) *sides* of the bar or frame*.

If we mark the vertex (or most elevated point) of the arc of any segment of a circle previous to its revolution about an inscribed vertical line touching the arc in any point, then will the true vertex and the initial mark coincide during the revolution; and the chord of the (vertical) segment (or a plane cutting it at right angles), if perpendicular to the vertical line, will continue horizontal throughout, and describe a perfectly horizontal plane. Now as the middle point of the bubble of a spirit-level will always come to rest at the vertex of the circular arch of its tube, represented by the arc of the segment; and as the intersection (at right angles) of the under surface of the level by the vertical plane of the circle of curvature is equivalent to the chord of the segment, (and that under surface to the plane cutting it at right angles,) we may be certain that the surface on which the level may be moved about in exact contact, without displacing the bubble, is a plane parallel to the horizon. Or, as the ends of the bubble do not deviate from their marks on the tube or divisions on the scale, the under surface of the level preserves during the revolution its parallelism to the surface of the liquid (or base of the bubble), which is always horizontal, and must therefore move parallel to a horizontal plane.

When the level rests on a horizontal plane, with the ends of the bubble coinciding with the two marks drawn on the tube, or with each end at the same distance from the zero of the scale, it is said to be adjusted; in which case the middle of the bubble is at the point of *bisection* of that arc of the circle

* If the circle C move with its centre on the circumference of the larger circle B, its plane being always perpendicular to that of the latter, and in the direction of the line joining their centres, then will its circumference generate a ring similar to the curved tube. The circle described by that point *v*, of the circumference of the smaller circle C, which lies in the direction of the plane of the great circle B (and of the straight line connecting their centres) will therefore represent the circle of curvature of the tube.



Having placed the tube with its axis parallel to the sides of the frame, bring the mark indicating the situation of some point of the circle of curvature to its greatest elevation above the under surface of the frame, when it will lie in the plane passing through the axis of the tube perpendicular to that under surface.

of

of curvature, of which the line of intersection of its plane by the (horizontal) under surface of the level is the chord.

As the sides of the level are perpendicular to its under surface, if we fix a short tube to the frame at right angles to the principal one, and mark the ends of its bubble when the level rests on a plane found to be horizontal, we shall know in future that the sides of the frame, and consequently the parallel plane of the circle of curvature, will be vertical when the bubble of the transverse tube comes to rest between its marks.

It is also evident, that if we place the level with either of its (perpendicular) sides in contact with a vertical plane so that its under edge (or corresponding longitudinal line of the under surface) lies exactly on, parallel to, or in the direction of a straight line described on that plane, and find on reversing the level (that is, on making the opposite side of the level to press against the vertical plane with its under edge coinciding with the straight line) that the ends of the bubble come to rest at the same marks, it proves that the line is horizontal.

If a segment of a circle be made to revolve about an inscribed line *inclined* to the horizon, and touching its arc in any point, its chord, if perpendicular to that line, will describe an inclined plane. A horizontal line being drawn on this plane through the point in which the line of revolution, if produced, would touch it, another straight line lying in the same plane and passing through that point at the greatest possible angular inclination to the horizon, also that of the plane, will intersect that horizontal line at right angles*. Then, if we mark the vertex of the arc when the segment is vertical, that is when in the direction of the inclined line drawn on the generated plane, the mark will attain its maximum distance from the true vertex, varying as the segment revolves, at the completion of a semi-revolution; at which period the reversed segment becomes vertical again, and coincides with the inclined line for the second time; and *half* this maximum distance, as measured on the graduated arc of the segment, will be equal to the zenith distance of the line of revolution, and to the horizontal inclination of the plane and generating chord. At one-fourth, and again at three-fourths of the revolution, the plane of the segment will be at its greatest inclination to the horizon,

* The intersection of an inclined plane through any point on it by a horizontal plane, is a straight line parallel to the horizon; and the intersection of the same plane by a vertical one passing through any point of it perpendicular to its horizontal (parallel) lines, will be a straight line having in common with lines parallel to it, an inclination to the horizon greater than that of any other line that can be drawn on the same plane.

equal to that of the inclined plane, when the chord, then in the direction of the horizontal line described on the inclined plane, will be horizontal, and the vertex be situated at the point of bisection of the total arc, and of that portion of it comprehended between the initial mark and the vertex at its greatest elongation from it. Or conceive an *adjusted* level to be placed so as to coincide in the first instance with any horizontal line drawn on the inclined plane, and subsequently with one at right angles to it, or in the direction of the line on which the inclination of the plane is measured; and it will be evident that the bubble must have advanced from zero towards the elevated end of the level, by a distance on the scale equal to the inclination of the plane. But on reversing the level, the same end, (repassing the horizontal line with the bubble reverted to zero,) will be depressed equal to its previous elevation, and the bubble must consequently come to rest at the same distance from zero as before, but on that side of it the nearest to the other (now elevated) end of the level; so that the arc of distance passed over by the bubble will equal *twice* the inclination of the plane.

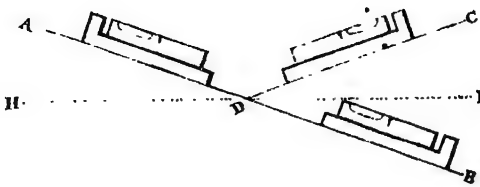
To find the angular inclination of a plane, move the level about on its surface until the bubble has made its maximum approach to the (elevated) end of the tube. Having registered the divisions on the scale giving the middle point of the bubble, proceed with the rotatory movement of the level until its direction is reversed, which will occur when the bubble has made its greatest approach to the other end of the tube, and ascertain the middle point of the bubble as before. *Half* the sum of these two middle points when the signs are unlike, or *half* their difference when the signs are like, will be equal to the angle of inclination of the plane*. When the level is placed on the inclined plane in such a direction that the bubble settles at that point of the scale answering to the half-difference of these two middle points, the longitudinal lines of the under surface of the level are horizontal, and its corresponding sides inclined at an angle equal to that of the plane†.

* When the level is furnished with a transverse tube properly adjusted, we are enabled to place it at once on the inclined plane, in the direction of its greatest inclination, which will be, when the sides of the level are vertical, known by the bubble of the transverse tube coming to rest between its marks.

† On this account the bubble would deviate (at right angles) from the equally inclined circle of curvature, and horizontal lines cannot be safely drawn by a similar level on the inclined plane with which its under surface is in contact. To draw them correctly, the sides of the level must be preserved vertical by means of the transverse tube, whilst one of the longitudinal lines of the under surface rests on the inclined plane with the bubble of the principal tube at the proper divisions.

The

The measurement of the angular inclination of a line lying in a vertical plane does not differ essentially from that of a line of maximum inclination described on an inclined plane, but is susceptible of more evident demonstration. Let AB and CD be two straight lines lying in the same vertical plane



and equally inclined to the horizontal line HH. Place the under edge of either (vertical) side of a level on AB, and, having marked the ends of the bubble, *reverse* the level, and make the (opposite) under edge coincide with DC; when the bubble, as the same end of the level is equally *elevated* as before, will come to rest at the initial marks. Depress this elevated end until the under edge coincides with AB, when the bubble must have passed over a distance on the scale equal to the angle CDB, evidently the *sum* of the two equal angles CDH and HDB, either of which is equal to the inclination of the line AB (or that of CD). In depressing the reversed level from its position on DC to that on AB, its under edge must have coincided with, or been parallel to the horizontal line HH when the bubble had run over *half* the distance answering to the double inclination of AB, equal angle CDB. Hence the under surface of the level will be horizontal when (its sides being vertical) the middle of the bubble settles at a point on the tube, or division on the scale corresponding to the point of bisection of the space run over by the bubble, on reversing the level on any inclined line. When this point falls exactly between the two marks drawn on the tube, or on the zero of the scale, the level is properly adjusted. When this is not the case, turn the adjusting screws until the bubble settles on the elevated side of zero at a distance from it equal to the inclination of the line on which the level rests. Should the error of adjustment be trivial, it is more advisable to ascertain and register its value every time the level is made use of, than to attempt to correct it by the screws; for variations of temperature not only cause the vertex of the tube to alter its position

 relatively

relatively to the under surface of the level *, but have also the effect of disturbing for some time the adjusting screws, and probably other parts of the mounting. On measuring by an adjusted level a number of planes or lines differing in inclination, the half-difference of the space passed over by the bubble should always be zero, or the instrument cannot be considered as perfect.

Leeds, March 5, 1827.

Erratum.—Page 260; *for* 200,000, *read* 206265; (the number by which the length of a division on the scale of a level answering to 1″ must be multiplied to obtain the length of the radius of its curvature).

LXXII. *A Synopsis of the Birds discovered in Mexico by* W. Bullock, *F.L.S. and H.S., and* Mr. William Bullock, *jun. By* William Swainson, *Esq. F.R.S. F.L.S. &c.*†

THE intercourse which recent political events have opened between Mexico and Great Britain, promises to be no less interesting to zoological science, than important to the commercial prosperity of both nations. Mr. Bullock was among the first of our countrymen, whose ardent curiosity led him to visit those distant shores; and the scientific treasures with which he returned, bear ample testimony to that zeal and indefatigable industry which has ever marked his pursuits. The exhibition of these objects, together with the valuable models and relics of Mexican antiquity, which this enterprising traveller procured during the short space of his sojourn, attracted the public attention for two years. That such a collection, invaluable to the historian as throwing a new light upon the ancient state of one of the most extraordinary nations of antiquity, should have been suffered, in these days, to have been dispersed by the hammer of the auctioneer, will excite the deep regret of every friend to knowledge. They may now, indeed, serve as objects of mere curiosity, but those advantages which the historian and the antiquary might have derived from their study and investigation, as a whole, are for ever lost.

The zoological subjects, possessed by no museum in Europe, shared the same fate, but not before Mr. Bullock had placed the

* When the temperature increases, the vertex removes to a point of the tube nearer the thicker end. In some levels now in my possession the variation is 1″ for every 2 degrees of Fahrenheit's thermometer.

† Communicated by the Author.

the whole in my hands, for the publicly avowed purpose of recording this portion of his discoveries. In the mean time his son, Mr. William Bullock, remained in Mexico; and, although occupied in more pressing avocations, continued to devote his leisure to the acquisition of its productions. His attention has hitherto been principally directed to the department of ornithology, in which he possesses considerable information. Every new remittance of specimens that he has since forwarded to this country, has tended to show how little is known of Mexican zoology. This, however, has been attended with some disadvantage to the task I had undertaken, inasmuch as it has delayed the publication of those descriptions which were made in the first instance.

That no further delay may take place, in securing the honour of these discoveries to Mr. Bullock and his son, I have drawn up, in this paper, a short synopsis of those birds which have reached me up to this time; indulging the hope of giving a more detailed account hereafter of the zoology of Mexico, accompanied by coloured figures.

It may readily be supposed, by those naturalists who have attended to the geographic distribution of animals, that in a country so new to science, many interesting objects would occur. The peculiar situation of Mexico, placed between the two great divisions of the American continent, and concentrating within itself every variety of climate, renders its zoology uncommonly interesting. The materials I have yet received are too scanty to allow of any very particular conclusions being drawn from them, so far as regards general views. I shall, therefore, merely observe in this place, that many of the ornithological groups of North America, occur also on the table land of Mexico, but that those of South America generally predominate. In addition to these are some few forms peculiar to the country itself, and one (*Cinclus*) which occurs in Europe, but in no other part of America.

The generic definitions will, I hope, shortly appear in another Journal, to which they have been sent, with the intention of preceding the publication of this paper, ever since last November. By this unfortunate delay, I am reduced to the unpleasant necessity of referring to a book not yet published, for what the reader should have the immediate power of consulting.

That this synopsis may be more generally useful to my English ornithological friends in Mexico, many of whom are miners, I have written the whole in our native language. It only remains for me to assure them, how much they have it in their power to benefit science, and to illustrate the natural history

history of that interesting country they have chosen as a residence, by devoting a portion of their leisure to this subject; and by giving me that assistance in investigating the productions of Mexico, which I can only hope to receive through their kindness.

February 1827.

ORDER. INSESSORES. Vig.

Family. Falconidæ.

1. *Harpya imperialis.* Cuv. Pl. Col. 14.

   A living specimen of this noble bird was in the possession of Count Regla, from which a spirited drawing was made by Mr. W. Bullock junior. It differs in some respects from the figure above quoted, which exhibits some indication of being taken from a young bird.

2. *Aquila.* A doubtful species, in immature plumage.

3. *Polyborus Braziliensis.* Ray. Vieil. Gal. des Ois. Pl. 7.

   This appears to be a common bird in Mexico.

4. *Circus rutilans.* Tem. Pl. Col. 25.

   A young bird, but identified with the *Falco rutilans* of M. Temminck by himself.

Fam. Hirundinidæ.

5. *Hirundo melanogaster.*

   Crown, back, scapulars, and spot on the throat glossy blue-black: front, throat, and sides of the head rufous: rump ferruginous: tail nearly even.

   Total length $5\frac{1}{4}$: wings $4\frac{1}{4}$; tail $2\frac{2}{10}$.

   Inhabits the Table land of Mexico. It has been since sent from Real del Monte.

   Breast dusky; body, vent and under tail covers white; collar round the neck gray; wings and tail brown; the first quill rather longer than the second.

6. *Hirundo thalassinus.*

   Above changeable green with liliac reflections; beneath snowy white; wings and tail violet brown; tail slightly forked.

   Table land; Real del Monte, by Mr. Morgan.

   Ears, sides of the head, and all the under parts pure white; wings long, the first quill longest.

   Total length, $4\frac{1}{2}$; wings, $4\frac{8}{10}$; tail, $2\frac{2}{10}$, depth of the fork $\frac{4}{10}$

Fam. Halcyonidæ.

7. *Alcedo Americana.* Lath. Pl. Entl. 591.

   Common on the Table land.

Fam. Muscicapidæ.

8. *Platyrhynchus pusillus.*

Olive

Olive brown, beneath yellowish-white; wings with two pale bands; tail moderate, even; bill small; head crested.

Maritime parts of Mexico.

There are four or five small American Flycatchers, perfectly resembling this in the colour of their plumage, but all differing very materially in the size and form of their bills. This, in the present bird, is rather broad, flat, and not abruptly hooked: when viewed in a vertical direction the margins appear rather dilated, or curved outwards; a character so conspicuous in the typical *Platyrhynchi*, that we may take it as a sufficient reason for bringing this bird within the confines of the same group.

The yellowish band at the base of the lesser quills is obsolete; the margins of the greater are not pale, neither is the outer feather of the tail margined with yellow.

Total length, $5\frac{1}{2}$: bill, $\frac{6}{10}$; wings, $2\frac{1}{2}$; tail, $2\frac{1}{4}$.

G. Tyrannula. *Swains. in Zool. Journ. No.* 10.

9. *Tyrannula affinis.*

Olive, beneath pale fulvous; wing covers and quills with pale margims; base of the lesser quills with a blackish band; bill small; under mandible yellow; tail divaricated.

Maritime parts of Mexico.

10. *Tyrannula obscura.*

Muscicapa querulæ? Vieil. Ois. de l'Am. pl. 39.

Above olive gray, beneath yellowish-white; wings short, brown, with two whitish bands; tail brown, even, the outer feather with a pale yellow margin.

Mexico. Rather larger than the last.

Total length, $5\frac{1}{4}$: bill, nearly $\frac{7}{10}$; wings, $2\frac{1}{2}$; tail, $2\frac{1}{2}$; tarsi, $\frac{6}{10}$.

11. *Tyrannula barbirostris.*

Brown, beneath pale yellow; crown blackish; chin and throat white; bill large, strongly bearded; tail even.

Mexico.

Total length, $6\frac{1}{2}$; bill, $\frac{7}{10}$; wings, 3; tail, 3; tarsi, $\frac{7}{10}$.

12. *Tyrannula nigricans.*

Blackish brown, head and throat darker; vent, under tail covers, and margin of the exterior tail feather, white.

Table land of Mexico: not uncommon.

Total length, 7: bill, $\frac{7}{10}$; wings, $3\frac{1}{2}$; tail, $3\frac{1}{4}$; tarsi, $\frac{8}{10}$.

13. *Tyrannula coronata.*

Muscicapa coronata. Gm. The most beautiful, and seemingly one of the most common species found in Mexico.

14. *Tyrannula cayenensis.*

Muscicapa Cayenensis. Gm. Maritime parts of Mexico.

15. *Tyrannula pallida.*

Pale

Pale gray, beneath ferruginous; throat hoary; tail black.
Table land of Mexico.
Total length, 7: bill, $\frac{7}{10}$; wings, 4; tail, $3\frac{1}{4}$; tarsi, $\frac{7}{10}$.

16. *Tyrannula musica.*
Cinereous-brown, beneath dirty yellow; tail forked; wings lengthened, brown; bill strongly hooked.
Total length, $7\frac{1}{2}$: wings, $4\frac{1}{4}$; tail, $3\frac{1}{2}$.
This bird may be placed either with the *Tyrannina*, or at the utmost limits of this group.

G. Setophaga. *Sw. in Zool. Journ. No.* 10.

17. *Setophaga ruticilla.*
Muscicapa ruticilla, Lin. *mas.* M. flavicaudæ. Gm. *fem.*
Maritime parts.

18. *Setophaga miniata.*
Cinereous, breast and body beneath vermilion; tail black, the lateral tail feathers partly white.
Table land: woods of Valadolid; rare, size of the last.

19. *Setophaga rubra.*
Entirely red, ear feathers of a silky whiteness.
Inhabits the same woods, and is of the same size as the last.

Fam. Laniadæ.

20. *Lanius Carolinensis.* Wilson iii. pl. 22. f. 5.
Table land: very common.

21. *Tyrannus intrepidus.* Vieil. Wilson ii. pl. 13. f. 1.

22. *Tyrannus griseus.* Vieil, Ois. de l'Am. pl. 46.

23. *Tyrannus sulphuratus.* Vieil. Swainson.
Maritime parts: with the two last.

24. *Tyrannus crassirostris.* Sw.
Maritime and table lands.

25. *Tyrannus vociferans.* Sw.

G. Ptiliogonys. *Swains. in Zool. Journ. No.* 10.

26. *Ptiliogonys cinereus.*
Cinereous; chin and middle of the lateral tail feathers white; under tail covers yellow; wings and tail shining black.
Table land of Mexico. Real del-Monte.

Fam. Merulidæ.

27. *Cinclus Mexicanus.*
Cinereous gray, head and chin brown.
Size of the European species.

28. *Merula migratoria.* Turdus migratorius auct. Wilson i. pl. 2.

29. *Merula*

29. *Merula flavirostris.*
Gray; back and wings tinged with ferruginous; beneath white; breast and flanks ferruginous; chin spotted; bill yellow.
Total length, $9\frac{1}{2}$: bill, 1 ; wings, 5 ; tail, $4\frac{1}{2}$; tarsi, $1\frac{2}{10}$.

30. *Merula tristis.*
Olive brown, beneath whitish; chin with black spots; under wing covers pale ferruginous; bill and legs brown.
Total length, 9 : bill, 1 ; wings, 5 ; tail, 4 ; tarsi, $1\frac{4}{10}$.

31. *Merula silens.*
Hermit thrush. Wilson v. pl. 43. f. 2.
Olivaceous gray, beneath white; chin, throat and breast with black spots; tail tinged with ferruginous.
This and the four preceding birds, are from Temascaltipec, on the Table land.
Total length, 7 : bill, $\frac{3}{4}$; wings, $3\frac{3}{4}$; tail, 3 ; tarsi, 1.

G. Orpheus. *Swains. in Zool. Journ.* No. 10.

32. *Orpheus polyglottos.* Turdus polyglottos. Wilson ii. pl. 10. f. 1.
Table land. Real del Monte.

33. *Orpheus curvirostris.*
Gray, beneath whitish; throat and breast spotted; vent pale fulvous; bill long, curved.
Table land.
Total length, $10\frac{1}{2}$: bill, $1\frac{1}{2}$; wings, $4\frac{1}{2}$; tail, 5 ; tarsi, $1\frac{1}{4}$.

34. *Orpheus cœrulescens.*
Bluish, crown and throat paler, ears and sides of the head black.
Table land. The notes of this species are very sweet.
Total length, $10\frac{3}{4}$: bill, $1\frac{1}{10}$; wings, $4\frac{1}{2}$; tail, $5\frac{1}{2}$; tarsi, $\frac{1}{10}$.

G. Seiurus. *Swains. in Zool. Journ.* No. 10.

35. *Sciurus aurocapillus.* Golden-crowned Thrush. Wilson ii. pl. 14. f. 2.
Table land?

36. *Seiurus tenuirostris.*
Above olive brown, beneath pale yellow with triangular blackish spots; stripe above the eye pale.
Table land? Size of the last.

G. Sialia. *Sw. in Zool. Journal.* No. 10.

37. *Sialia azurea?* Sylvia Sialis? Wilson i. pl. 3. f. 3.
Common on the Table land at Real del Monte and other places. I have some doubts whether this is not a distinct species. my specimen is of a young bird.

[To be continued.]

LXXIII. *Outlines of a Philosophical Inquiry into the Nature and Properties of the Blood; being the Substance of three Lectures on that Subject delivered at the Gresham Institution during Michaelmas Term* 1826. *By* JOHN SPURGIN, *M.D. Fellow of the Royal College of Physicians of London, and of the Cambridge Philosophical Society.*

[Continued from p. 207.]

WHEN blood is drawn from the body, as from a vein of the arm, it presents the obvious characters of fluidity and redness, and to the touch is warm and slightly viscid; after remaining at rest, however, for about seven minutes, it begins to separate into two distinct portions; the one yellowish and fluid, and occupying the surface of the mass; the other red and solid, and tending to the bottom of the vessel that contains it. This change is denominated the coagulation of the blood; the watery part is termed the serum, and the red and denser part the crassamentum, coagulum, or clot.

In order to our obtaining a more particular or intimate knowledge of these now separate parts of the blood, we must avail ourselves of the experiments and observations which have been recorded by various authors upon whom we can place the greatest reliance. We learn then from these, that the serum is an apparently homogeneous fluid of a yellowish colour, unctuous to the touch, and saline to the taste; that its specific gravity is very variable, but on the average is about 1029, water being 1000; whilst that of blood fresh drawn, and therefore in its more natural state, is about 1050. When exposed to a heat of 160° of Fahrenheit, it is converted into a somewhat firm white mass which is designated coagulated albumen, and which on being cut into slices and subjected to gentle pressure, gives out a small quantity of a slightly opaque liquor, of a saline taste and peculiar odour, which is called the serosity, consisting of water, pure soda holding albumen in solution, of muriate of soda, or common salt, muriate of potash, slight traces of phosphoric acid, besides lactate of soda, and animal matter. When serum is evaporated at a heat below that required for its coagulation, it yields a yellowish semitransparent mass resembling amber, that splits into pieces in drying, and amounts to about 95 grains from 1000 of serum. But not only is the serum permanently coagulated by heat, but also by the mineral acids; and the insoluble compounds thence produced exactly resemble those of the same acids with the fibrin of the crassamentum, on which we shall have to speak presently: and as alcohol produced similar effects with both these matters,

ters, the celebrated chemist Berzelius was led to contend that there was very little difference between these two products, albumen and fibrin.

The only character that appears to distinguish one from the other, is, that whilst albumen requires a high temperature for its coagulation, the fibrin will coagulate spontaneously at a low one. Berzelius states the composition of the serum to be 905 parts water; 80 albumen; substances soluble in alcohol, such as the muriates of potass and soda, 6 parts; lactate of soda and animal matter, 4 parts; and substances soluble in water only,—such as soda, phosphate of soda, and a little animal matter,—5 parts. In this statement of the composition of the serum, nothing is said of the existence of sulphur in any of its forms or modifications: but other chemists have given such proofs of its existence in the serum of the blood, as to render it beyond all doubt. Alcohol, metallic salts and tan, will also cause its coagulation; and the same change, according to the discovery of Mr. Brande, may be effected by the negative wire of the voltaic electric circle.

Dr. Bostock is of opinion that some of these agents in the coagulation of the serum, as alcohol, and perhaps the stronger mineral acids, produce their effect by abstracting a portion of the water which held the albumen in solution: while tan and the metallic salts unite with the albumen and form a compound which is insoluble in water, and consequently separates from the fluid; thus producing an effect, which should rather be styled precipitation than coagulation *. When serum is coagulated, it exactly resembles the white of the egg hardened by boiling; and it is found to be essentially the same with this substance, whence it has obtained the name of albumen. The efficient cause of this coagulation is a question that has been frequently discussed †, and the only way it can be accounted for, is, by supposing some change to take place in the figure or nature of its particles, by which their relation to each other is altered; but what the nature of this new relation is, or the means by which it is effected, has not been explained. Concentrated sulphuric acid coagulates albumen; but if assisted by a moderate heat dissolves it again and forms a solution of a very fine red colour. Several hypotheses have been framed to account for this remarkable change; but the abstract conclusion just now stated, cannot be disputed. Before coagulation, serum is readily miscible with, or soluble in water; but after coagulation it is completely insoluble. A most remarkable effect takes place after digesting coagulated albumen for some time in diluted nitric acid; for it is hereby converted

* Bostock's Elements of Physiology, vol. i. pp. 408, 409. † Ibid. p. 470.

into a substance which possesses the physical and chemical properties of jelly*.

With regard to the red and denser part that forms or separates spontaneously from the more fluid part or serum, which in contradistinction to the serum is called the cruor or clot, it appears to the eye, generally, under the form of a soft solid, of such consistence as to bear cutting with a knife, and to be of a fibrous and reticulated structure. The colour of this fibrous mass may be washed away or separated by repeated ablutions in water, and the mass will then be found to be made up of a white shred-like matter, apparently deriving its colour from a substance distinct from itself, or from a colouring ingredient which is only mechanically mixed with it, and not retained by any chemical affinity. When this shred-like matter is thus procured in a pure state, it is found to be a solid of considerable consistence, elastic and tenacious, and in its general aspect as well as in its chemical relations very similar to the pure muscular fibre. It has been designated by several names; as the coagulable lymph, gluten, fibre of the blood, and fibrin. Upon this substance the spontaneous coagulation of the blood depends. Many are the experiments which have been made with the view of ascertaining the circumstances which peculiarly affect or produce this spontaneous change of the blood, or which influence it in any way either to retard or to promote it: but it is not requisite to adduce in this place all the modes resorted to for the purpose, nor all the results that have been obtained; but suffice it to say, that rest chiefly conduces to its coagulation, whilst free agitation and the addition of certain neutral salts will either retard it or prevent it altogether. The experiments of Mr. Hewson on the coagulation of the blood are very interesting, inasmuch as they show that this tendency is modified by various circumstances, even in the living body; and he was enabled to conclude from them, that any thing which tended to impair the strength of the body,—as large bleedings, faintings, &c.—seemed to increase the tendency to coagulation †; as also certain passions of the mind,—the depressing passion fear more especially. Whilst, on the contrary, any thing which increased the action of the vessels, or in other words excited the body, lessened this tendency very considerably: indeed, his experiments led him to think that the properties of the blood depend on the action or state of the blood-vessels, or "that they have a plastic power over it, so as to be able to change its properties in a very short time ‡."

In inflammations, when the blood-vessels are acting more

* Bostock's Elements of Physiology, vol. i. p. 473.

† Hewson's Experimental Inquiries, p. 126.

‡ Ibid. p. 127.

strongly,

strongly, Mr. H. says, the disposition of the fibrin to coagulate is proportionably diminished. On the contrary, when an animal is bled to death, or when the vessels are acting with the lowest degree of strength, the blood was more and more disposed to coagulate in proportion as the animal was reduced. Temporary exertion of strength, moreover, even the struggles of the dying animal, he thought might lessen this tendency for a short time; for the struggles of dying sheep, he says, seemed to alter the lymph or fibrin: and he adds, that although it must be admitted it is very difficult to conceive how the blood-vessels should do this, yet he hopes that ingenious men will not merely on that account reject his conclusion; but would consider, that as it is deduced from a number of experiments, as it agrees with all the appearances, and as it leads to an explanation of many of them which we cannot otherwise account for,—it may be well founded, although it be difficult to be conceived. For there may be powers in the animal œconomy that are not yet dreamt of in our philosophy.

Mr. Hewson spent much labour and time on these points; and we find that he confirms the testimony of some of the writers of the last century, in stating that the sulphate and muriate of soda and the nitrate of potash were among the most powerful salts in counteracting this change; so much so indeed, as Dr. Bostock has quoted, that if we add to a portion of blood rather less than 1-20th of its weight of any one of them, the coagulation does not take place: on which remarkable effect Dr. B. remarks, that this cannot be owing to any tendency in the salt employed to dissolve the fibrin, because the neutral salts do not possess this property; at the same time that potash, which is the proper solvent of fibrin, has *less* power in retarding its coagulation.

The mere dilution of the blood with a sufficient quantity of water will effectually prevent its spontaneous coagulation, which is attributed to its particles being thereby removed to so great a distance from each other as to be placed beyond the reach of their mutual attraction. The coagulability of the fibrin is also affected and indeed destroyed by electricity and lightning; for in persons who have been killed by lightning, the blood is not coagulated at all, but remains perfectly fluid: nor is this the only remarkable effect; their muscles also remain as flexible as ever, and the rigidity and stiffness which is the common consequence of death does not take place*. But what may be regarded as more extraordinary still, this coagulation of the blood is likewise prevented by a blow on the pit of the stomach when causing sudden death, instances of

* Henry's Elements of Chemistry, vol. i. p. 320.

which

which occasionally happen. The same occurs when death is occasioned by the poison of the viper; or by injury to the brain; or by some vegetable poisons, as laurel-water; or by violent passions of the mind, or by over-exertion of the body. In some diseases, on the contrary, its tendency to coagulation is greatly increased. The striking difference that is observable between blood drawn from a person in full health and from one who is labouring under inflammation, has excited the interest and inquiries of many medical men and chemists; which difference consists in the surface of the coagulum being of a yellowish, buffy, or leather-like appearance, instead of a dark or florid red. Numerous hypotheses have been broached to account for this appearance. The immediate cause of this appearance in the crassamentum, says Dr. Bostock, is obvious: the globules, *or other matter which give it the red colour*, begin to subside before the coagulation is completed, so that the upper part of the clot is left without them; the remote cause not being yet ascertained.

Dr. Dowler made some experiments on the composition of the buffy coat: and from these it appears that it contains a very large proportion of serum, which by diminishing the viscidity of the crassamentum, must more readily allow of the subsidence of the red or colouring particles. The appearance of a buffy coat on the surface of the blood is usually regarded as indicative of the inflammation of some organ; but as it occurs in other states of the body, other discriminating signs are sought after by the physician, to be convinced of its existence. The blood taken from a pregnant female most commonly exhibits this buffy coat: or collecting the blood in different vessels during the same bleeding will be followed by the extraordinary circumstance,—that the first portion will exhibit no such appearance; the second will exhibit it in a considerable degree; the third more or less so, and so on; which changes Mr. Hewson is inclined to attribute to certain alterations in the state and action of the blood-vessels; whilst more recent observations discover that the same results are brought about by the more or less free escape of the blood from the body, by the size of the orifice made in the vein, or by the form of the vessel the blood is received into. In short, these results all prove that the coagulation of the fibrin is liable to alteration from various causes, or from the operation of various influences. The difference in specific gravity between the fibrin and serum varies according to numberless circumstances; for though in general the former subsides to the bottom of the vessel containing them, yet it must be confessed it is sometimes seen to be floating in the serum, and nearly on a level with its surface;

surface; consequently, its specific gravity differs very much in different cases. The chemical properties of fibrin appear exactly to resemble those of the muscular fibre; being acted upon in the same manner by nitric acid and the other re-agents, so as fully to be entitled to the appellation of liquid flesh, as bestowed upon it by the older physiologists; and the great resemblance between the muscular fibre and the sanguineous, has led many to imagine that they are in fact identical. Alcohol of sp. gr. ·810 converts the muscular fibre into a kind of adipocirous matter—into a substance partaking of the properties both of fat and of wax; whilst this is more completely and perfectly effected by ether, yielding it in greater abundance, and distinguished by a much more disagreeable odour. It is converted by means of heated acetic acid into a tremulous jelly, becoming immediately soft and transparent, which jelly is dissolved by warm water, with the evolution of a small quantity of azotic gas. Strong muriatic acid boiled on fibrin, decomposes it, and produces a red or a violet-coloured solution; digesting it with weak muriatic acid, it becomes hard and shrivelled: this by repeated washing in water changes at length into a gelatinous mass that is perfectly soluble in tepid water. This solution reddens litmus paper, and yields a precipitate with acids as well as with alkalies. Strong sulphuric acid decomposes and carbonizes fibrin; diluted with six times its weight of water and digested with fibrin, it acquires a red colour, but dissolves scarcely any thing. The undissolved portion is a compound of fibrin with an excess of sulphuric acid; and when this excess is removed by water, a *neutral combination* is obtained, which is soluble in water, and has the same characters as the neutral compound of fibrin and muriatic acid.

Strong nitric acid at first disengages nitrogen or azotic gas from fibrin, pure and unmixed with nitrous gas. By continuing the digestion twenty-four hours, the fibrin is converted into a pulverulent mass, of a pale citron colour, which when placed on a filter and washed with a large quantity of water, becomes of a deep orange colour.

This yellow substance was discovered by Fourcroy and Vauquelin, who gave it the name of yellow acid. Berzelius has ascertained that it is a combination of the nitric and *malic* acids with fibrin, which is in some degree altered by the process. Fibrin precipitated from its solution in caustic alkali has undergone some change by the solution; for it is now insoluble in acetic acid. The changes that are wrought on the blood by means of various other chemical substances, it would be both prolix and useless to mention: what we have adduced

is

is sufficient to prove that the blood is a fluid *sui generis*, though a compound of many chemical elements.—We must now speak of the red globules, or of the *colouring matter of the blood.*

For some time these have been regarded as a distinct component part of the crassamentum, as well as the cause of the red colour of the blood: for when the structure of the crassamentum is viewed by the microscope, it not only appears fibrous or thread-like, but also reticulated, the interstices of the network being occupied by a greater or less number of red globules, and by a quantity of the serum or watery part; the effect of the coagulation being to entangle a part of the red globules and serum: whence, if the coagulum be sliced and cut into small pieces, a quantity of serum makes its escape, in addition to what was separated from the mass of blood at the time of its coagulation. Now, as the red globules are the heaviest part of the blood, they subside to the lower part of the clot or coagulum at the time of its formation; and consequently it might be inferred that if the blood coagulates very slowly, these globules will have an opportunity of subsiding completely from the upper portion of the clot, and the clot will thence be devoid of its red colour; and as the blood which is drawn from a person labouring under severe inflammation, is covered with a yellowish or buffy crust, it is imagined that this is an effect of the slower coagulation of the fibrin allowing the red particles to subside more completely. But a material objection to this theory is, that in most instances the coagulation is actually accelerated in inflammations: indeed we have repeatedly seen the blood coagulating before the bleeding has been stopped, and assuming the buffy surface; whilst at other times, again, the coagulation has been very slow, but no buffy appearance was exhibited. These effects, then, rather seem to depend upon some altered condition of the blood itself.

[To be continued.]

LXXIV. *On the Chlorides of Lime and Soda. By* R. Phillips, *F.R.S. L. & E. &c.*

THESE chlorides have lately excited considerable notice, owing to the recommendation of M. Labarraque; and not only in France, but in this country also: should they be found to possess a moderate share only of the powers which have been attributed to them, they are exceedingly important compounds, and their exact nature as well as their mode of action is intitled to a more careful examination than, as appears to me, they have hitherto received.

The

The chloride of lime has long been known under the name of oxymuriate of lime, or bleaching powder: it is prepared, as is well known, by passing chlorine gas over hydrate of lime; the resulting compound, put into water, yields the chloride of lime in question; it is also sometimes formed by passing the gas into water containing lime in suspension: this chloride is now proposed to be employed, and it appears with great success, as a disinfecting substance.

The existence of such a compound as chloride of soda, or potash, has hitherto not been so clearly ascertained as that of the chloride of lime. Two modes of preparing this compound have been proposed,—one by M. Labarraque, and the other by M. Payen; the former passes chlorine gas into a solution of carbonate of soda, while the latter proposes to decompose chloride of lime by carbonate of soda.

I have tried both methods, and both are very easy of execution. When chlorine gas is passed into the solution of carbonate of soda, it is very readily absorbed, and this absorption takes place without expelling a particle of carbonic acid: the solution has a faint smell of chlorine; when heated scarcely any chlorine is evolved, and the solution first acts as an alkali upon turmeric paper, and then bleaches it: on the addition of an acid, chlorine and carbonic acid gases are evolved.

When evaporated until a slight pellicle appears, a mass of fibrous crystals is soon formed, the consistence of which is almost pulpy, owing to the retention of the solution by the capillary attraction of the crystals. When these crystals have been separated, the solution yields minute crystals of carbonate of soda, possessing the usual form of that substance.

These filamentous crystals are too minute to admit of any examination of their form: they appear to me to consist of chlorine, carbonic acid, and soda, or chlorine in combination with carbonate of soda; when put into a solution of indigo in sulphuric acid, they immediately decolorize it, by the evolution of chlorine, accompanied with carbonic acid. I have not yet had an opportunity of analysing this compound; I find, however, that whilst drying by exposure to the air, it loses so much chlorine,—I presume by the action of carbonic acid,—that it does not contain two per cent. of it in any state, either of mixture or combination. I have not particularly examined the solution formed by decomposing chloride of lime by means of carbonate of soda; but I find that it retains its bleaching power after ebullition, and yields crystals by evaporation.

In the last Number of the Phil. Mag. and Annals, an account was given of a paper of Dr. Granville's, read before the Royal Society, on the composition and action of the chloride of soda. According to Dr. Granville, the disinfecting properties of chloride of soda are entirely dependent upon

the uncombined chlorine gas which the water holds in solution. Now admitting for a moment this to be the case, and that no such compound as chloride of soda exists, the same explanation cannot apply to the action of chloride of lime: and it is singular that Dr. Granville has taken no notice of this substance, although according to M. Labarraque it is generally employed for disinfecting apartments, while the chloride of soda "is especially employed in topical and external applications to wounds and ulcers affected with hospital gangrenes, &c." (Alcock on the Use of the Chlorurets, p. 126.)

Some late experiments have proved in a most decided manner that the explanation which Dr. Granville has offered, is not a correct view of what actually occurs. M. Gaultier de Claubry has shown, that air which was passed through putrid blood and afterwards into a solution of chloride of lime, was rendered inodorous, and was completely purified, occasioning the precipitates of carbonate of lime; but in a similar experiment the fœtid air was passed through a saturated solution of caustic potash; the chloride of lime had then no effect upon it, and it retained its insupportable odour: this is decisive as to the action of the carbonic acid of foul air in evolving the chlorine by which it is purified.

I have already mentioned that chloride of soda does not even by ebullition lose its bleaching property; and this is another proof that its action does not depend upon the mere gas which it holds in solution; for it will hardly be maintained that any circumstance less than combination will retain chlorine in water at a boiling temperature. It also retains its power to a considerable extent even after evaporation to dryness.

Dr. Granville states, that the salt in question is a mixture of 73·53 of chloride of sodium and 28·47 of chlorate of soda; I am quite at a loss to conjecture how Dr. Granville obtained this result, either by calculation or experiment. For preparing the chloride of soda M. Labarraque directs that a solution of 288 parts of crystallized carbonate of soda, is to have the chlorine evolved from the decomposition of 66 parts of common salt passed into it.

Now as 288 are equivalent to 2 atoms of crystallized carbonate of soda, there will be required the chlorine of 2 atoms = 120 of common salt to convert them into chloride of sodium and chlorate of soda; and even admitting, what I believe is not the case, that the chlorine of the 66 parts of the common salt, converts, as far as it goes, the carbonate of soda into chlorate of soda and chloride of sodium, the quantity is so deficient that the dry salt must consist very nearly of

Chloride of sodium .	5
Chlorate of soda . .	[illegible]
Carbonate of soda . .	39

LXXV. *Notices respecting New Books.*

Elements of Chemistry, including the recent Discoveries and Doctrines of that Science. By EDWARD TURNER, *M.D. F.R.S.E., &c. &c.*

DR. TURNER is already advantageously known to the chemical student as the author of an "Introduction to the Study and Laws of Chemical Combination," &c. and the present work will not detract from his merit as a clear, and in general a correct, narrator of the numerous facts and theories embraced by, and constituting, chemical science; indeed it would be difficult to name any work of similar extent on the subject, which contains so much information.

After a few pages of introductory matter, relating principally to the physical properties of bodies, and the definition of chemical science, Dr. Turner divides his work into four principal parts. In the first of these, the imponderables are treated of; the second comprises inorganic chemistry, including the doctrine of affinity and the laws of combination; the elementary bodies are divided into nonmetallic and metallic,—and the author has deviated, and we think with great propriety, from the usual practice of dividing elementary bodies into supporters of combustion and combustibles, or into electro-negative and electro-positive bodies: we agree with him, that an arrangement founded on these principles is not free from objection in theory, and that it offers no advantage in facilitating the study of the science. The third general division of the work is on organic chemistry, comprehending the products of vegetable and animal life; while the fourth division contains brief directions for the performance of analysis.

Although our limits will not permit us minutely to follow Dr. Turner through the details and execution of his plan, we shall offer such observations upon many parts of it which may appear to require notice; and if the author should find that we are free in our remarks, we trust he will receive them in the same spirit as that by which they are dictated; and we hope we may not only be useful to him in a second edition, which we have no doubt will soon be required, but we trust that utility may also arise to those who may possess the work in its present form.

The Imponderable first treated of is Caloric. The chapter is written with clearness and precision. If we do not mistake, alcohol is not, as Dr. T. asserts, the only fluid which has not been solidified; for if we remember right, neither sulphuret of carbon, chloride of azote, nor protochloride of carbon, have ever been rendered solid; and we recommend him to peruse Mr. Daniell's observations in vol. xxi. of the Royal Institution Journal, before he repeats his statement as to the novelty and utility of Mr. Jones's Hygrometer.

In treating of Light, the second imponderable, Dr. T. observes that terrestrial light has been supposed to contain no chemical rays. In opposition to this opinion we may remind the author, that Mr. Brande has shown (Phil. Trans. 1820), that the intense light pro-

 duced

duced by galvanic action is similar to solar light: this was proved by its causing the rapid combination of chlorine and hydrogen gases; indeed, Dr. T. afterwards admits that the similarity is confirmed by the chemical effects recently occasioned by phosphorescent or more correctly, incandescent lime.

The chapters on Electricity and Galvanism do not call for any particular observation: we would merely remark, that the different theories of the pile are given with great brevity and clearness.

To the subject of galvanism, succeed some remarks on the methods of taking specific gravities, and on chemical nomenclature: and then follow affinity and the changes that accompany chemical action; the subject of crystallization is dispatched with most unusual brevity, the primary forms of crystals are not even mentioned; indeed we suspect both from this circumstance and some of the author's subsequent statements respecting crystalline forms, that they have received but little of his attention.

In treating of Cohesion, Dr. Turner offers some observations, to the accuracy of which we can by no means assent. Thus muriate of lime is stated to be decomposed by carbonate of ammonia, in consequence of the insolubility of the carbonate of lime. Now as insolubility is a property arising from combination, it cannot be the cause of it: it is unquestionably true that insolubility may, and certainly does in many instances, prevent chemical action; but it appears to us to be utterly confounding cause and effect, to imagine that a property which a body would possess when formed, can be the cause of its production: supposing also that insolubility were the cause which produced the carbonate of lime, it ought also to prevent its decomposition by muriatic acid, and the carbonates of lead. lime, and barytes ought to be as insoluble in any acid as their respective sulphates.

We cannot agree with Dr. Turner, that if four substances be mixed together, the compound which is insoluble will, in all cases, be formed; the fact that prussian blue and peroxide of mercury, both substances insoluble in water, yield by boiling in it a soluble cyanuret of mercury, is a sufficient refutation of this statement as a general law.

Again; the author observes that some substances are decomposed on account of their volatility: this argument is similar to that respecting the insolubility, and is refuted by the same reasoning; viz. that causes cannot act previously to their existence: if the volatility of a body in its uncombined state, were to influence it while in combination, hydrate of potash ought to be as easily decomposed as hydrate of copper; and indeed the existence of hydrate of potash at a red heat completely disproves another of Dr. Turner's statements, viz. that all compounds which contain a volatile and a fixed principle, are liable to be decomposed at a high temperature.

The most remarkable exception to the accuracy of this statement is the compound of chloride of phosphorus and ammonia, discovered by Davy: this substance, composed of three elementary gases,

gases, and a very volatile solid, is not only difficult of decomposition, but is not volatilized at a red heat; many similar examples might be quoted.

In refuting an error respecting the action of quantity in modifying affinity (p. 114), it appears to us that the author has not been fortunate in the selection of his example:—Thus he states, that "acids and alkalies have a tendency to unite in more than one proportion, and will readily form salts with excess of acid or of base when circumstances are favourable to their production." If by this it is meant that the alkalies form compounds with acids which contain less than one atom of acid, we do not remember any such case; but we readily admit the formation of salts with excess of acid: and this, according to Dr. Turner, "explains why nitrate of potash cannot be entirely decomposed by a quantity of sulphuric acid; which is just sufficient for neutralizing the alkali: the sulphuric acid," he continues, "instead of taking the whole of the potash, unites with a part of it and forms the bisulphate. This tendency to the formation of an acid salt accounts for the fact quite satisfactorily; nor is there any reason to infer the co-operation of any other cause." We are enabled by the results of direct experiment to show the inaccuracy of these statements. When 100 parts of nitre were decomposed by the equivalent quantity of sulphuric acid, more than $\frac{92}{100}$ of the whole quantity of nitric acid were procured, and the salt left in the retort weighed 86·2 parts, which is the precise atomic quantity of sulphate of potash obtainable by decomposing the nitrate by its equivalent of sulphuric acid: that it contained no bisulphate of potash, was proved by its being rendered alkaline by $\frac{1}{80}$ of its weight of carbonate of potash: it is scarcely necessary to add, that if two atoms of sulphuric acid were required to decompose one atom of nitrate of potash, only $\frac{50}{100}$ of the whole quantity of nitric acid could have been procured, instead of $\frac{92}{100}$; the loss of 8 parts being inevitable in the operation. When treating of nitric acid, we shall briefly resume the subject of employing two atoms of sulphuric acid to decompose one atom of nitre.

We have already alluded to Dr. Turner's "Introduction to the Study and Laws of Chemical Combination;" this work the author has judiciously introduced, with a few alterations, into the present volume. A clear and excellent view is given of the subject; but we shall notice two or three important errors,—mere slips of the pen,—two of which also occur in the original treatise. First, in p. 135, we have 05554, instead of 0·5554; and 9720, for 0·9720: and in page 148, the ratio of the oxygen of the acid and of the base in neutral sulphates, is stated to be as one to three, instead of three to one.

The third section commences with an account of the properties of Oxygen; and upon some of the statements which it contains, we shall offer a few observations. Dr. Turner has correctly stated the sources from which oxygen is procurable: he might, however, have added deutoxide of lead to the list. He agrees with Dr. Thomson in stating, that 44 parts = 1 atom of peroxide of manganese, lose 4 parts of oxygen by exposure to a red heat and become deutoxide.

Dr

Dr. Thomson has also mentioned that if the peroxide be exposed to too high a temperature, it loses more oxygen,—not indeed sufficient to convert it into protoxide, but there is obtained a compound of 1 atom of protoxide with 2 atoms of deutoxide. Now Dr. Turner says, that by the action of sulphuric acid "the peroxide loses a whole proportion of oxygen, and is converted into protoxide, which unites with the acid, forming a sulphate of the protoxide of manganese." This statement is at variance with both Dr. Thomson's and also with the results of our experiments; for we find that 44 or 1 atom of peroxide of manganese yield 4·2 of oxygen, which is so much nearer 4 than 8, that there is no question but that the deutoxide and not the protoxide is obtained by the action of sulphuric acid; that this is the case is further proved by the deep red colour of the solution of the sulphate, and by its losing that colour, as stated by Dr. Thomson, when mixed with sulphurous or nitrous acid.

In describing the properties of oxygen gas, the author states that it does "not evince a disposition to unite with acids or alkalies." Perhaps not with them, as such, and yet it combines with sulphurous, nitrous, and other acids, and with potash, soda, and other alkalies; and the compounds which it forms are not divided by chemists into acids and oxides only, but into acids, oxides, and alkalies. Hydrogen is the second elementary body in Dr. Turner's arrangement. We do not deny, but we most certainly doubt, the production of carbonic acid when iron is dissolved in dilute sulphuric acid: the metal unquestionably contains charcoal,—but whence proceeds the oxygen necessary to its conversion into carbonic acid? This chapter contains but few other passages which call for observation, excepting the assertion that zinc cannot decompose water at common temperatures is contradicted both by Davy's experience and our own. It takes place with great slowness, but still it actually does occur. We do not find any thing material to arrest our progress till we arrive at Nitric Acid (p. 195); and here intending to resume our remarks on this subject, we shall first quote Dr. Turner's statement, that "there are two substantial reasons for using more than one proportion of sulphuric acid to one of nitre. The first is, that nitre cannot be wholly decomposed by a quantity of sulphuric acid, which is merely sufficient to form a neutral sulphate. Owing to the tendency of potassa to unite with two proportions of that acid, the product would contain a portion of bisulphate and of undecomposed nitre." This part of the subject we have already disposed of; but by referring to Wollaston's admirable paper on Chemical Equivalents, Dr. Turner will find the true reason for employing 2 atoms of oil of vitriol for decomposing 1 atom of nitre: it is, that there may be sufficient water for condensing the nitric acid; for while 1 atom of sulphuric acid $= 40$ is condensed by $9 = 1$ atom of water, 1 atom of nitric acid $= 54$, requires 2 atoms of water $= 18$ for its condensation; the formation of the bisulphate of potash is a consequence merely of employing as much sulphuric acid as contains the requisite quantity of water to condense the nitric acid. The other reason for employing 2 atoms of

of sulphuric acid for decomposing 1 atom of nitre, is stated by Dr. Turner to be the facility which it affords of removing the residual salt from the retort;—this we will admit: he continues, however: "but though it is advisable to use more than 1 atom of sulphuric acid, it is important to employ no more than is really required for decomposing the nitre with advantage. An unnecessary excess," he continues, "is not only uneconomical, but positively hurtful; for some of it is then apt to pass off in vapour during the distillation, and thus render the nitric acid impure. The proportions of the Edinburgh College are calculated to fulfil all those conditions; the excess of sulphuric acid is sufficient to decompose almost all, if not the whole of the nitre, and a pure nitric acid is obtained."

Now it appears to us that some of these statements are at variance with others: the proportions employed by the Edinburgh College, are two parts of sulphuric acid and three of nitre: if then, according to Dr. T.'s statement, "nitre cannot be wholly decomposed by a quantity of sulphuric acid, which is merely sufficient for forming a neutral sulphate," the proportions of the Edinburgh College do not and cannot "fulfil all the conditions" alluded to, nor is the "excess of sulphuric acid sufficient to decompose almost all the nitre;" and the residue of the Edinburgh process, instead of being "a mixture of the sulphate and bisulphate of potassa," as Dr. Turner states, must, according to his own showing, be a compound of about 128 parts of bisulphate of potash and 45 of nitre. It may also be observed that when 2 atoms of sulphuric acid are employed, no portion of it ever rises and contaminates the nitric acid; nor indeed can it do so, consistently with the opinion that it is necessary to combine with the potash of the nitre.

As far as we have observed, there are a few, and only a few statements which require notice, in looking through a large further portion of Dr. Turner's work. We would, however, inquire whether it would not be a more correct expression to speak of *evolving* than of *forming* hydrogen and other elementary gases; a mixture of nitric oxide and hydrogen gas burns with a greenish and not with a white flame (p. 187). It may also be difficult to conceive how an orange-coloured liquid, like nitrous acid, should give different shades of green and blue merely by being diluted, and yet green muriate of cobalt becomes red by dilution, and green muriate of copper blue by the same means. We do not by these observations mean to deny the probability of Dr. Turner's suggestions, at p. 195, respecting the nature of the changes produced in nitrous acid by mere dilution; all we intend is to show that the case is not without a parallel. We do not know that it is of much consequence that the same nomenclature should in all cases be adopted; but on a subject which is always puzzling to learners,—we mean the atomic theory,—it rather increases the difficulty to use numerous terms in the same sense, especially when treating of any peculiar substance. In p. 210, Dr. Turner states that carbonic oxide is regarded as a combination of one *proportion* of carbon = 6 and one of oxygen = 8; and carbonic acid of one *atom* of carbon = 6 and two of oxygen = 16. The

The *combining proportion* of carbonic oxide is therefore 14, and that of carbonic acid 22. The italics are ours; and we use them to mark the circumstance that three different terms are used in the space of as many lines, to express the same meaning. In p. 215, a similar case occurs.

In p. 217, the sulphuric acid obtained by decomposing sulphate of iron is stated to be colourless: the fact is well known to be otherwise; but we believe the cause of the dark colour has not been ascertained: Dr. Thomson, if we remember right, suspects that it may be derived from the presence of selenium.

We proceed now to the Metals, the arrangement of which, if we may use the expression, is too chemical, and not sufficiently dependent upon obvious or physical properties. The metals are divided into two classes; First, those the oxides of which cannot be reduced to the metallic state by the sole action of heat; Secondly, those metals the oxides of which are reducible by heat only. As however the metals of the first class are divided into four orders, the arrangement is in fact equivalent to five classes. The first order includes the metals which decompose water at common temperatures; these are stated to be six, viz. Potassium, Sodium, Lithium, Barium, Strontium, and Calcium. Now to those ought to be added, Magnesium from the second order, and Zinc and Manganese from the third order. That the two former metals decompose water at common temperature, is stated by Davy. The action of manganese we have witnessed.

The second order of metals includes Magnesium, Glucinum, Yttrium, Aluminum, Zirconium, and Silicium.

One of the characteristics of these metals is stated to be, that their oxides are very sparingly soluble in water; but on referring to the account given of each of them, three of them, viz. alumina, glucina, and zirconia, are stated by Dr. Turner himself, to be "quite insoluble in water;" and the same might also have been mentioned with respect to yttria. The late experiments of Berzelius have nearly demonstrated that neither silicium nor zirconium has any claims to be considered as metals: indeed two circumstances mentioned by Dr. Turner with respect to silicium,—viz., that it is a non-conductor of electricity, and has no metallic lustre,—are almost decisive with respect to that substance.

The third order includes those metals which decompose water at a red heat; they are stated to be Manganese, Zinc, Iron, Tin, and Cadmium. Now, as already noticed, two of these decompose water at common temperatures; and according to Davy, antimony produces the same effect at a red heat. The remaining order includes metals which do not decompose water at any temperature; and the second class contains those, the oxides of which are decomposed at a red heat.

From the length to which our observations have already extended, we shall be unable to allot more than a very limited space to the remaining parts of Dr. Turner's work. We may however observe, that his account of the crystalline form of muriate of barytes will, we think, justify an observation we have previously made respecting the little attention which Dr. Turner has paid to the subject of crystalization.

crystallization. Thus at p. 356, muriate of barytes is said to crystallize in four, six, or eight-sided tables, without any reference to its primary or secondary forms; and the form of a six-sided prism, bounded by pyramids with six sides, in which sulphate of potash is said to crystallize, is not merely a secondary one, but a macle. In p. 358, line 7, we have 1·8 parts or one atom of water, instead as we presume, 12 atoms; and in the next page, line 14, calcium occurs instead as we suppose, of strontium; and in p. 262, chloride of calcium, is substituted for chloride of lime.

There are some statements respecting the oxides and salts of manganese which require careful revision; and although, as we have had frequent occasion to show, Dr. Turner's work bears evident marks of occasional haste, it will in general be found a safe guide for the student. We would advise the author, however, to give more rules for conducting processes; and towards the latter part of the work especially, the compositions of bodies are too frequently omitted. And we cannot help observing, that the graphic illustrations are not sufficiently numerous, and are altogether unworthy of the work.

LXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 22.—THE reading of Mr. Whewell's paper On experiments made at Dolcoath mine in Cornwall, to determine the density of the earth, was concluded; and Professor Airy's Appendix to it also read.

The experiments described in this paper were made with two invariable pendulums, one swung at the surface of the ground at Dolcoath, the other at the depth of 1220 feet in the mine. The prosecution of them beyond a certain point, and their complete verification by a second exchange of the pendulums between the two stations, was rendered impossible by the accidental destruction of one of the pendulums. Their result, however, down to this point, gives 8″·23 per day, for the sum of the variations or double variation, of each of the pendulums, observed above and below. And Mr. Whewell considers that this result is the inevitable consequence of the observations; although very much greater than could have been anticipated, from any opinion hitherto entertained of the earth's internal structure and density. In reasoning on this result the author concludes, that it would require a mean density of the earth equal to 7·73 times that of the superficial stratum, or about 20 times that of water, to produce the difference of 8″·23 observed: that had it been only 6″, still a density of 13 times that of water would be required; but that the usual estimate resulting from the calculations of Hutton and Zach, would reduce the difference to 2″·46, a quantity too distant from that observed, to be attributable to any error of their observations. Neither can this difference be attributed to local attractions, or to the removal of the matter in the mine; but considering the uncertainty, under which, from the accident above mentioned, the results labour, Mr. Whewell regards any exact cal-

culations as superfluous, and recommends a repetition of the experiments with the experience already gained, and in the same locality.

The Appendix to this paper by Mr. Airy contains the formulæ of computation used, and their application to the observed cases. The observations were made by the method of disappearances and re-appearances, the vibrations being continued till the arc of vibration had become so small as to produce a complete obscuration of the disk, for more than 40″ in some instances, and thus entailing errors on the method of disappearances alone, which Mr. Airy characterizes as most enormous, as he also states to be, the interval between the disappearance on one side and the disappearance on the other, as the arc of vibration diminishes. It therefore became necessary to find some rule for determining the exact time of coincidence, which was found to be a mean of the first disappearance and the last re-appearance, or that of the first re-appearance and last disappearance; or rather a mean of all these four times. Mr. Airy next considers the correction depending on the arc of vibration, and then, successively, the thermometric and barometric corrections; and he finally states at length the analytical theory of them all.

March 29.—Viscount Mahon, and the Rev. C. Mayo, were respectively admitted Fellows of the Society. And the reading was commenced of a paper On the compounds of chromium; by Thomas Thomson, M.D. F.R.S.

LINNÆAN SOCIETY.

April 3, and April, 17.—The reading of Mr. W. S. MacLeay's paper On the Birds of Cuba was continued on the above evenings.

GEOLOGICAL SOCIETY.

Feb. 16.—At the Anniversary Meeting of the Society, held this day, the following gentlemen were elected Officers and Council for the year ensuing.

President: William Henry Fitton, M.D. F.R.S.—*Vice-Presidents:* Arthur Aikin, Esq. F.L.S.; John Bostock, M.D. F.R.S.; Rev. W. D. Conybeare, F.R.S.; Rev. Adam Sedgwick, F.R.S. Woodwardian Professor, Cambridge.—*Secretaries:* W. J. Broderip, Esq. F.L.S.; R. I. Murchison, Esq. F.R.S.—*Foreign Secretary:* Henry Heuland, Esq.—*Treasurer:* John Taylor, Esq. F.R.S.—*Council:* Henry Thomas De la Beche, Esq. F.R. & L.S.; J. E. Bicheno, Esq. Sec. L.S.; Davies Gilbert, Esq. M.P. V.P.R.S.; George Bellas Greenough, Esq. F.R. & L.S.; John Frederick William Herschel, Esq. Sec. R.S.; Armand Levy, Esq.; Charles Lyell, Esq. F.R.S.; William Hasledine Pepys, Esq. F.R.S.; Rev. John Honeywood Randolph; Charles Stokes, Esq. F.R.S. & L.S.; J. F. Vandercom, Esq.; Henry Warburton, Esq. M.P. F.R.S.; Thomas Webster, Esq.; Thomas Young, Esq.

March 2.—A paper was read, "On the volcanic district of Naples;" by G. Poulett Scrope, Esq. F.G.S. F.R.S.

In this paper the author purposes to confine himself to a general view of the volcanic formation of this district, and to such observations as have hitherto escaped notice, or on which he differs from other writers.

At

At one extremity of the tract in question lies the habitually eruptive volcano of Somma; at the other the once active vent of Ischia; the intermediate space is studded with hills, evidently thrown up by numerous eruptions, succeeding one another at distant intervals, and from separate though neighbouring orifices. These are arranged in one general band, which is remarkable from its parallelism to the elevated limestone range forming the opposite side of the Bay of Naples, and separating it from that of Salerno.

Somma is a very regular volcanic mountain, created by the accumulation of repeated streams of basaltic lava and beds of ejected ashes, sand and scoria, round a central and habitual vent.

The author dissents from the theory of Von Buch, that such mountains were produced by the forcible elevation of horizontal beds round an aperture of eruption;—though he allows that beds originally inclined, may often suffer a certain degree of elevation, during the shocks occasioned by the forcible protrusion of lavas from below, into the fissures through which they are emitted.

The great crater of Somma is attributed to the explosions of the "paroxysmal eruption" of A. D. 79; and the whole cone of Vesuvius which occupies the centre of that crater, is stated to have been created by repeated subsequent eruptions. This cone is similar in structure to that of Somma, as is seen in the walls of its actual crater, compared with those of the Atrio del Cavallo.

Ischia is a less regular volcanic mountain; has produced no leucite, and none but trachytic, or rather, according to the author's nomenclature, gray-stone lavas,—a class intermediate between trachite and basalt, and consisting of felspar and augite. The great mass of the island is composed of the conglomerates belonging to this class of lavas, forming an indurated tufa of a light green colour. There are traces of a vast central crater on the west of the Monte Epomeo. Some of the lavas of Ischia are remarkably brecciated and zoned,—with varieties of grain, texture, and mineral composition.

The intermediate district between Somma and Ischia, properly called the Campi Phlegrei, including the islands of Procida and Nicida, exhibits the traces of between twenty and thirty crateriform basins, many of very large diameter, but in general much degraded, and sometimes almost obliterated, by the erosive action of the sea and of rains on the loose conglomerates of which they are partly composed, and by the ejections of later neighbouring eruptions. Ten at least of these cones, with their included craters, are however, very nearly entire; such are the Monte Nuovo, produced in the year 1538; Capo Mazza, a hill entirely composed of silky pumice and its detritus; the Monte Gauro, which incloses a deep circular crater a mile in diameter; Astroni, which is nearly equal to the last in size, and precisely similar in figure; the basins of the lakes Averno and Agnano; the island of Nicida; the southern extremity of the island of Procida; the Capo di Miseno; and the Solfatara of Pozzuoli.

The author disputes the existence of any large vaulted cavity under the floor of the last-mentioned crater; and attributes the rever-

 beration

beration produced when it is struck sharply, to the cellular nature of the beds of indurated clay which form this floor, and have been deposited from the washings of the surrounding slopes, and hardened by the influence of heat and moisture.

The author accounts for the production of two varieties of Pisolite, which occur in the tufa and decomposed lava of the Solfatara. This hill is recorded to have been in eruption in A. D. 1180; and the present crater may have been formed at that late epoch. The hill which supports the Camaldoli, 1643 feet above the sea, is a remarkable mass of indurated tufa; from beneath which, on the N. E. side, crops out a bed of gray-stone, in which a singular concretionary separation has taken place, of the augitic from the felspathose parts; the former appearing as lenticular patches in a base consisting of the latter. This and other somewhat similar lavas in the same neighbourhood, give rise to important inferences as to the condition of such substances at the period of their emission from the earth. The solid tufa of Capo di Monte and other hills envelops shells of the same species with those which at present inhabit the Bay of Naples. It is likewise in some points traversed by vertical veins of a finer and harder matter, seeming to have exuded from the sides of a fissure formed in the rock, before it was completely desiccated.

The author attributes the formation of all these volcanic hills to successive eruptions from below the surface of the sea, though on a shallow shore: and, from the existence of loose tufa over the whole plane of the Campagna, and even to some distance up its principal valleys, he infers that the sea once washed the foot of the Appenines behind Capua; and that this plain has since suffered an elevation of 200 feet at least,—an elevation in which the whole western coast of Italy and the Appenines probably shared; as appears from the traces of lithophagi in the cliffs between Rome and Palermo, much above the present sea-level, and from other collateral testimony.

March 16.—A paper was read "On the geology of the vicinity of Pulborough, Sussex;" by P. J. Martin, Esq.

The author's object is to give a detailed account of the district on the north of the South Downs, extending from about Petworth on the west, to Steyning and the Adur on the east, and intervening between the portions of Sussex described by Mr. Mantell and Mr. Murchison. The structure of this tract agrees in general with part of the adjoining district on the west; but two of the formations are here subdivided into natural groups, which the author conceives ought to be distinguished; the following being the series in a descending order, that has come under his observation:—1. *Chalk.* 2. *Firestone,*—including upper greensand and Malm-rock. 3. *Galt.* 4. *Shanklin sand,*—including, as subdivisions, ferruginous sand, and lower greensand and sandstone. 5. *Weald clay.*

The portion of the *Firestone*, which the author denominates Upper Greensand, may be traced distinctly as a thin bed at the foot of the chalk hills from Sutton to Washington, and is best exposed at the entrance of the Arundel defile, resting upon the Malm-rock,—an argillaceous

argillaceous limestone which extends into terraces in some places 50 feet thick and half a mile in breadth. The *Galt* is probably not more than 60 feet in its greatest thickness: it is widest on the E. of Sutton, and from thence eastward varies in width from a few hundred yards to a quarter of a mile. The upper, or ferruginous, portion of the *Shanklin sand*, occupies the broadest space between the chalk and the weald, and is from one to three miles in width, its northern boundary forming a very distinct escarpment. The surface of these sands is distinguished by its barrenness; they vary much in consistency and colour, and the lower beds especially, are pervaded by seams of clay, and abound in a stone consisting of coarse siliceous sand cemented by oxide of iron. The lower division of this formation (green sandstone) has in some portions a strong external resemblance to the stratum immediately beneath the chalk. It constitutes a fertile arable country, and affords pure and copious springs. The upper part contains thick layers and nodules of limestone, chert, and clay resembling fuller's earth. The lower affords a compact building-stone, which has long been quarried at Pulborough: but further west, these beds pass into chert. This stratum has obviously suffered great disturbance; and one of its natural chasms, forming the valley of Greenhurst, and about 4 miles in length, points towards the outlet of Arun, and might probably be taken advantage of to connect that river with the Adur. The demarcation between the lower part of the *Weald clay* and the subjacent *Hastings' sands*, is not well defined in the tract which the author describes. A considerable bed of sand occurs within the clay at its upper part; after this comes in a bed of "Sussex marble;" and lower down in the clay, a second layer of sand containing siliceous grit in thin beds; beneath which the principal beds of Sussex marble (about 18 inches in thickness) occur; and these are finally succeeded by blue, brown, and red clay, and micaceous sand, the commencement of the forest ridge.

The author gives a particular description of the defile of the Arun, the principal outlet of the Weald in the south of Sussex. This river traverses about 15 miles of a country almost mountainous, cutting across the ridges of the sand and the chalk escarpment nearly at right angles to the valley of the Weald. The gorge, where it enters the green sandstone, is more than 400 or 500 yards in width at the bottom; and the banks rise quickly to the height of about 200 feet on the east; and on the west to about 400 or 500 feet. At Bury and Amberly, where the river penetrates the chalk, the hills are 600 or 700 feet high; the ravine having all the characters of a fissure. And as the strata, in several cases of this description, rise on both sides towards the crack, the author supposes that the channels now existing on the surface, have been produced by the operation of some internal forces by which the beds were broken up and elevated; and that the drainage of the country by the present outlets, can be thus explained; without having recourse to a debâcle, or to denuding operations: and he supports this hypothesis by reference to the local features of the country, illustrated by sections.

ASTRONOMICAL SOCIETY.

April 11.—A paper by Colonel Beaufoy, was read, containing his Observations of Eclipses of Jupiter's Satellites, from January 2 to May 15, 1826, together with some Observations of occultations of stars by the moon.

A paper was also read "On the Longitude of Madras, as deduced from Observations of Eclipses of the first and second Satellites of Jupiter, taken between the years 1817 and 1826. By John Goldingham, Esq. F.R.S."

The eclipses stated in this paper are 96 in number, being Immersions and Emersions of the 1st and 2nd Satellites only. Of these, 11 are directly comparable with those of Colonel Beaufoy, made at Bushey Heath, viz. 8 of the 1st, and 3 of the 2nd; and their mean result, which of course is independent of the errors of the tables, is stated by Mr. Goldingham at $5^h\ 21^m\ 9^s{\cdot}3$, being the longitude of Madras east of Greenwich. The remainder, consisting of 34 Emersions and 35 Immersions of the first Satellite, and 12 Emersions and 4 Immersions of the second, are not directly comparable with Colonel Beaufoy's. Mr. Goldingham endeavours, however, to render them so, or at least to eliminate the errors of the tables, by determining the latter from Colonel Beaufoy's observations made nearly about the same time, and then applying it to the results of a comparison of his own with the Nautical Almanac as a correction; and in this way deduces a conclusion agreeing almost exactly with the foregoing.

This is not the place to enter into any discussion on the legitimacy of the process pursued by Mr. Goldingham for this purpose, or of its general applicability in the present state of the tables. The end of this abstract will be better answered by presenting in one view the results of these several classes of observations as obtained separately, by direct comparison with the Nautical Almanac, *uncorrected* by reference to Colonel Beaufoy's, or any other observations, which may be stated as follows:

Madras East of Greenwich.

By 34 Emersions of the 1st Satellite observed at Madras, and compared with the Nautical Almanac.	5^h	21^m	$6^s{\cdot}5$
By 35 Immersions of ditto, similarly observed and compared.	5	21	12·4
Mean Longitude of Madras	5	21	9·4
Difference of Immersions and Emersions			5·9
By 12 Emersions of the 2nd Satellite, similarly observed and compared	5^h	21^m	$0^s{\cdot}5$
By 4 Immersions of ditto	5	21	33·1
Mean Longitude	5	21	16·8
Difference of Immersions and Emersions			32·6

The latter series has, however, only the weight of four double observations, and is therefore no way to be put in competition with the former, corroborated as it is to minute precision by the results of

of the comparative observations; so that on the whole we may take $5^h\ 21^m\ 9^s{\cdot}35$ as the true longitude of the Madras observatory.

Mr. Goldingham states the difference of longitudes between the Observatory and Fort St. George at $2'\ 21''$ (of space), the latter being to the east; so that the longitude of Fort St. George, Madras, is $5^h\ 21^m\ 18^s{\cdot}7$.

Immediately after the conclusion of the ordinary meeting, a Special General Meeting was held, pursuant to notice, for the purpose of presenting the Honorary Medals awarded by the Council; on which occasion the President, J. W. F. Herschel, Esq. delivered a very admirable and interesting Address, which we regret that our limits will not enable us to insert till next month.

HORTICULTURAL SOCIETY.

March 6.—The following paper was read: Upon forcing garden rhubarb; by Mr. W. Stothart.—A fine specimen of the *Anastatica hierochuntica* was exhibited by A. B. Lambert, Esq.; and the table was covered with a profusion of flowers, fruit, and vegetables. Various seeds and cuttings were distributed.

March 20.—It was stated from the chair that H. R. H. the Duke of Clarence had been graciously pleased to signify his desire of becoming an Honorary Member of the Society, in the vacancy occasioned by the death of his late R. H. the Duke of York.—The following papers were read: An account of the varieties of the apple which have been found to succeed in Ross-shire, in latitude 57° 34′ N; by Sir George Stewart Mackenzie, Bart.—Upon the best mode of raising seedling fruit-trees; by Mr. Weissenborn, of Weimar.—Various flowers and fruits were exhibited, especially specimens of some remarkably fine apples, called the Farleigh Pippin, which were in excellent condition notwithstanding the lateness of the season.—The usual distribution of seeds and cuttings was made.

April 3.—The following papers were read: On a new method of obtaining strawberry plants for forcing; by Mr. Alexander Diack.—Upon the cultivation of running kidney beans for forcing, in preference to those commonly used; by the Rev. George Swayne.—A variety of seeds and cuttings were distributed. Upon the table were some fine specimens of oranges, lemons, and limes, from Malta; and some apples called the Bess Poole, and the Thoresby Seedling, the names of which deserve record from their great beauty and excellence. They were in as high perfection on this day, as any apple could have been at Christmas. There was also some fine fruit of the Beurrée de Pâques, commonly called the Paddington Pear; a variety now at its best.

ZOOLOGICAL SOCIETY.

We are happy to perceive that this Society is now completely organized. A meeting was held in March for electing a President in the place of the late lamented Sir Thomas Stamford Raffles, when the Marquis of Lansdown was unanimously elected to that office. Regular weekly meetings of the members have since taken place

on each Wednesday, between the hours of one and five, for the purpose of inspecting the museum. This department consists of extensive and instructive collections in every branch of zoology; the value of which is enhanced by the consideration of their being exclusively the result of the liberality and public spirit of some of the leading members. Lectures on various subjects of interest in zoology have also been given at 3 o'clock during these meetings. Mr. Vigors, the Secretary, has delivered some discourses on the Affinities of Birds, illustrated by specimens from the Society's museum; and Mr. Brookes has commenced a series of lectures on Comparative Anatomy, selecting the structure of the Ostrich as the subject of the first lecture on Wednesday the 25th. Several valuable preparations from a specimen of this bird, which His Majesty was graciously pleased to present to the Society, were brought forward to illustrate this lecture. The gardens of the Society are stated to be in great forwardness, and it is expected that they will be opened during the ensuing summer.

ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 23.—Dr. Harwood read a paper from the lecture-table, On the natural history of the seal. It was illustrated by numerous fine specimens and preparations of the animal, from the collection of Mr. Brookes and other sources, and by many specimens of the fur and skin, in the state into which they are brought by the furrier. Dr. Harwood particularly remarked upon the amazing expansion of the olfactory nerves in the Seal, from which, and other circumstances, he was induced to suggest as probable, that these animals often hunt their prey upon the surface of the water by scent. He expressed his surprise too, that they had not as yet been brought under subjection to man, and made to perform the same good offices for him in the water that the dog does on land.

In the library were the contents of a Tumulus found near the Falls of Niagara, Upper Canada, and of another in the back settlements of Ohio: several Egyptian antiquities, presented by General Tolly; and new American and English publications.

March 2.—The librarian, Mr. Singer, read a paper communicated by a member of the Institution, On the principles of the structure of language. The investigations had been made upon the Hebrew tongue, and were illustrated by numerous diagrams.

The head of the Ghurial of the Ganges, with other fine specimens in Natural History, and presents of books, were laid upon the library tables.

March 9.—Mr. Holdsworth made some observations in the lecture-room on the structure of Shipping: they were merely introductory to some practical and experimental illustrations which are to be submitted to the members.

In the library was a specimen of gas for illumination, made by Mr. Daniell's process from resin. Resin, tar, and numerous other sources of highly carbonated combustible gases have hitherto been objectionable only because during decomposition they choke the retorts

retorts and pipes with carbonaceous matter. Mr. Daniell's process obviates this objection, and renders these substances readily, œconomically, and in consequence advantageously, available.

Several new works of art, and presents, were laid upon the tables, with some very ancient and scarce books.

March 16.—Mr. Ainger gave a brief sketch of the history and principles of suspension bridges. Simple communications of this kind were observed to be very numerous and ancient, especially in Asia; but in them the road was laid on the curved lines of suspension, whereas in the suspension bridges of the last twenty years it has been hung from these lines in such a manner as to form a horizontal surface. The credit of the latter application was given to America. The causes of the great superiority in œconomy, of suspension over insistent bridges, was pointed out; and the peculiar advantages united in iron over those of any other substance explained. The direction and amount of the forces exerted, the necessary strength required to sustain them, and many other circumstances in their construction, were then developed and explained by models and diagrams.

Numerous specimens of Natural History and of expensive and rare literary works were laid upon the library tables.

March 23.—Mr. Reinagle of the Royal Academy delivered a discourse On the property of beauty contained in the oval. The peculiar beauty of the ever-varying curve of the oval was pointed out, and the manner in which, by the combination of different parts of different ovals, arranged according to a certain order, all the forms of Grecian vases might be obtained. A still more extended application of the principle was made to the grouping of the important parts of figures and objects in historical paintings:—this was illustrated by reference to the works of the great Italian painters.

In the library were worked specimens of Swedish porphyry:* some specimens of pierced metallic plates, the holes being made at perfectly regular intervals, and accurately punched, so that no bur was produced, or any circumstance created to destroy their uniformity; some of them were so fine as to present the appearance almost of an uniform metallic surface.—Some peculiar crystalline depositions from oil of turpentine, with numerous new books, were also upon the tables.

March 30.—The subject this evening was A general view of the animal œconomy, particularly illustrated by a history of the circulation in man and other animals; by professor Pattison.

A large meteoric stone was placed on the library table, with a particular account of its fall, in the Persian language. This was translated by Dr. Wilkins. The stone fell in the night of the 7th of August 1822, near the village of Kadonah, in the district of Agra. It descended with much noise as of cannon and of the wind, awakening those who were asleep, and alarming a watchman who heard it fall. On making search in the morning, the stone was found warm, and with a little smoke rising from it: it is to be subjected to examination.

Mr. Ritchie's simple and accurate Balance was also placed upon

the table; likewise specimens of the Pemmican, which have been prepared by government for Capt. Parry's voyage; an experiment upon the reflection of light at different angles; and new publications.

April 6.—A few observations were made by Mr. Webster on the impulse of wind on sails.

In the library were several fine presents to the Museum of Natural History; amongst which was the skeleton of the Ouran Outang of Borneo, given by Gen. Hardwicke. On the tables were specimens of paper made from various substances; books presented to the library, and various literary novelties.

The evenings were then adjourned over two Fridays, to April 27.

LONDON MECHANICS' INSTITUTION.

On Wednesday the 4th of April, Professor Millington completed a course of lectures on Pneumatics at this Institution, to an exceedingly crowded auditory of the members. Mr. Kirby commenced a course of lectures on the Steam Engine, on the 11th of April. These lectures will be succeeded about the 23rd of May, by a short course on Luminiferous animals, on Phosphorescence, and on the Philosophy of the ordinary means of producing fire; by Mr. E. W. Brayley, jun. The Friday evenings have been occupied with lectures on Prejudice, by Mr. Chambers; on Combustion, by Mr. Hemming; on the Architectural Antiquities of Britain, by Mr. Stackhouse; and Dr. Birkbeck (the President,) commenced his course on the Structure and Functions of the Human Body, last Friday evening, the 27th of April.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Sittings of June 22, 1826, to the 17th of Feb. 1827.

The President announced that after an examination of the precedents for the prizes founded by M. de Montyon, for Experimental physiology, no decision excluded memoirs on the physiology of vegetables.

M. Arago communicated a letter from M. Boussingault, addressed to M. de Humboldt, and dated Bogota, in which this traveller describes the earthquake that occurred in that city on the 17th of June 1826.

MM. Dulong and Gay-Lussac reported on the Memoir of M. Dumas on some points of the atomic theory. "We trust," says the reporter, "that we have sufficiently shown the importance of the recent researches of M. Dumas. They contain that talent for observation, exactness of methods of experimenting and justness of views which characterize his other labours. We propose therefore to the Academy, to bestow its approbation on this Memoir, and to order its being printed in the *Recueil des Savans étrangers*."

M. Geoffroy Saint-Hilaire read A report, drawn up by himself, MM. De Lamark and Boyer, on a Memoir by M. Vincent Portal, D.M. entitled *Description de plusieurs monstruosités humains anoncéphales*. The Memoir was approved, and advised to be printed in *Recueil des Savans étrangers*.

MM. Thenard

MM. Thenard and Chevreul reported on a Memoir of M. Sérullas on the constituents of bromine. "Bromine, with which M. Balard has lately enriched chemical science, has so great analogy with chlorine and iodine, that it forms similar combinations with other bodies: this results from the experiments of M. Balard, and it is confirmed by the new results of M. Sérullas. These results are the production of hydrobromic æther and a cyanuret of bromine, which are obtained like hydriodic æther and cyanuret of iodine, and which they resemble very much in their appearance and properties. Hydrobromic æther is a colourless liquid, heavier than water, very volatile, of a strong æthereal odour, a sharp taste, very soluble in alcohol, from which it is precipitated by water.—As to the cyanuret of bromine, it crystallizes in long fine needles, it is colourless, very solid, a very pungent smell, and acts so strongly upon the animal œconomy, that a grain of the cyanuret dissolved in water is sufficient to kill a rabbit. Added to this, in all the experiments to which the cyanuret of bromine has been subjected, M. Sérullas did not observe any circumstance which induced him to think that bromine is a compound body. M. Sérullas not only repeated before us the principal experiments which relate to the hydrobromic æther and the cyanuret of bromine, but he made others which he had tried since he sent his memoir to the Academy, which show that bromine solidifies at 20° below 0 (Centigrade), that it acts strongly upon hydriodide of carbon, and that the result, attended with great heat, is a bromide of iodine soluble in water, and a hydrocarburet of bromine almost insoluble in it, which has an æthereal smell and a sweet taste." M. Sérullas's memoir is recommended to be printed in the *Recueil des Savans étrangers*.

LXXVII. *Intelligence and Miscellaneous Articles.*

ON BROMINE: BY M. SÉRULLAS.

M. BALARD having stated that bromine does not become solid at 0° of Fahr., M. Sérullas intended to employ liquid sulphurous acid as a cooling medium for its solidification. Having however, as a preparatory step, subjected some bromine to a cooling mixture of about 4° below zero, it became solid and very hard in an instant, and was broken by a blow: the experiment succeeds very well by putting the bromine in a watch-glass.

When one part of hydriodide of carbon is added to two parts of bromine in a glass tube, the former is immediately decomposed with the extrication of much heat, accompanied with a hissing noise: there are formed bromide of iodine, and liquid hydrocarburet of bromine; a portion of bromine is therefore substituted for iodine; water dissolves the bromide of iodine and the hydrocarburet of bromine coloured by bromine, separates at the bottom of the liquor; the colour is removed by the necessary quantity of potash.

If the hydriodide of carbon is in excess, but little hydrocarburet of bromine is formed; but a subbromide of iodine is obtained, from which iodine is precipitated by the careful addition of solution of potash.

 Hydro-

Hydrocarburet of bromine after washing with potash is colourless, heavier than water, of a penetrating æthereal smell, an extremely sweet taste which it imparts to the water that covers it, and in which it is but slightly soluble; it is extremely volatile, and remains solid at 40° to 42° Fahr., and is about as hard as camphor. M. Sérullas obtained hydrobromic æther by distilling from a small tubulated retort a mixture of 38 parts of alcohol, one part of phosphorus, and 7 or 8 parts of bromine, gradually added. When the bromine came into contact with the phosphorus under the alcohol, rapid action took place with the extrication of heat, and hydrobromic and phosphorous acids were formed. These are to be distilled with a gentle heat, and the product is to be received in a well-cooled receiver. The distilled liquor being mixed with water, the hydrobromic æther separates. If any acid be present, the washing water is to have a small quantity of potash added to it

Hydrobromic æther is colourless, and transparent after long standing; it is heavier than water, and has a sharp taste and a strong æthereal odour. It is very volatile, and soluble in alcohol, from which it is precipitated by water. It does not suffer any alteration of colour, as hydriodic æther does when kept under water.

CYANURET OF BROMINE.

M. Sérullas obtains this by the following process.—Put at the bottom of a small tubulated retort, or a long glass tube, two parts of dried cyanuret of mercury, that there may be excess of it. This long tube is to be placed in cold water or a freezing mixture, and one part of bromine is then to be added; the action is very considerable; so much heat is given out, that without the artificial cooling, the high temperature would prevent the bromine from coming into contact with cyanuret of mercury; bromuret of mercury, and cyanuret of bromine are formed; the latter crystallizes in the form of long needles in the upper part of the tube, surrounded by a little vapour of bromine which disappears by causing it to condense, and fall back upon the cyanuret of mercury.

A small well cooled receiver is then adapted to the orifice of the tube, into which by the application of a slight degree of heat, the cyanuret of bromine rises and crystallizes in cubes or needles. Cyanuret of bromine so strongly resembles cyanuret of iodine in its physical properties, that they may be readily confounded, especially when the former is acicular: the cyanuret of bromine has a penetrating smell, more so even than the cyanuret of iodine; and it is also more volatile; it becomes aëriform at about 15° below zero (Centigrade), and suddenly crystallizes by cooling. Cyanuret of bromine is more soluble in water and alcohol than cyanuret of iodine. Solution of potash converts it into hydrocyanate and hydrobromate of potash. This solution, when nitrate of silver is added, gives a precipitate of cyanuret and bromuret of silver, which are easily separable, the latter being soluble in ammonia, and the former not. The different experiments to which the cyanuret of bromine was subjected, never deprived the bromine of its characteristic

istic properties, even in those cases which would be the most likely to separate its elements, if it were not a simple body.

Cyanuret of bromine is extremely deleterious. A grain dissolved in water and given to a rabbit, instantly killed it; the inconvenience produced by its deleterious properties, as well as the scarcity of bromine, induced M. Sérullas to discontinue his experiments upon it.—*Ann. de Chim. et Phys.* tom xxxiv. p. 95.

NEW THEORY OF CRYSTALLIZATION.

Accident has within the last few days thrown in our way a volume on Mineralogy, by Mr. Beudant, published in 1824. On looking through it, we have been surprised to observe a statement which is as wholly inaccurate in fact, as it is inconsistent with theory.

In page 60, Mr. B. states, that "in all crystals of which the composition is identical, the angles are the same; but if the crystals contain accidental mixtures, the angles are sensibly and constantly different." He says, "the angles we find in these cases are generally the mean of the angles of each of the mixed substances taken proportionally to the quantity of each.

"Thus if 10 particles of carbonate of lime be mixed with one of carbonate of magnesia, the angle of the compound will be the 1-11th part of the sum of 10 angles of 105° 5′ and one of 107° 25′, which would be 105° 24′ 10″.

"I have," he adds, "observed these angles for these compositions, and Mr. Mitscherlich has made similar observations." And he adduces in support of this statement the known angle of "carbonate of lime and magnesia, 106° 15′, as the mean of 105° 5′ and 107° 25′."

But it unfortunately happens that the carbonate of magnesia from different localities distinctly measures 107° 30′.

The statement therefore fails in the only instance in which its correctness otherwise could be ascertained; that is, in the case of a *definite* compound. And there can be no doubt that both Mr. B. and Mr. M. have been deceived by their goniometer in the measurements of merely *mixed* minerals which Mr. B. supposes he has obtained

The instances in which mere mixture does not alter the crystalline form of a mineral are too numerous to admit of doubt as to the fact; and carbonate of magnesia is known to exist with various proportions of carbonate of iron, yet without the slightest appreciable variation in its angle of 107° 30′.

If the fact were as Mr. Beudant supposes, where in the crystal does he imagine the single atom of carbonate of magnesia should be placed, supposing 10 atoms of carbonate of lime, or 20, or 100 atoms, so as to affect the angular measurement of the crystal?

NEW PATENTS.

To Aristides Franklin Mornay, of Ashburton House, Putney Heath, for improvements in preparing for smelting and in smelting ores, or in extracting metals from such ores.—Dated the 27th of March 1827.—6 months allowed to enrol specification.

To

To Matthew Bush, of Dalmonach, Print Field, near Bonhill, by Dumbarton, calico-printer, for improvements in machinery or apparatus for printing calico and other fabrics.—27th of March.—6 months.

To Bennett Wodcroft, of Manchester, manufacturer, for processes and apparatus for printing and preparing for manufacture yarns, of linen, cotton, silk, woollen, &c.—31st of March.—6 months.

To Henry Astuey Stothert, of Bath, founder, for improvements on or additions to ploughs.—4th of April.—6 months.

To John Paterson Reid, Glasgow, for improvements on power looms —4th of April.—6 months.

To J. Tilt, of Prospect-place, St. George, Southwark, for improvements in salt-pans, and in the mode of applying heat to the brine.—4th of April.—6 months.

To E. Cowper, of Clapham-road Place, Surrey, for his improvements in printing music.—5th of April.—6 months.

To J. S. Broadwood, of Great Pulteney-street, Westminster, for certain improvements in grand piano-fortes.—9th of April.—6 months.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·30 Mar. 19. Wind SW.—Min. 28·79 Mar. 4. Wind SW. Range of the mercury 1·51.

Mean barometrical pressure for the month	29·716
——— for the lunar period ending the 27th instant	29·723
——— for 16 days with the Moon in North declination . .	29·480
——— for 14 days with the Moon in South declination . .	29·966
Spaces described by the rising and falling of the mercury . . .	11·440

Greatest variation in 24 hours 0·940.—Number of changes 31.

Therm. Max. 60° Mar. 24. Wind NW.—Min. 32° Mar. 4. Wind NW. Range 28°.—Mean temp. of exter. air 74°·06. For 30 days with ☉ in ♓ 44·92
Max. var. in 24 hours 24°·00 - Mean temp. of spring water at 8 A.M. 48°·54

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 1st	96°
Greatest dryness of the air in the afternoon of the 31st	44
Range of the index	52
Mean at 2 P.M. 60°·7—Mean at 8 A.M. 69·6—Mean at 8 P.M.	71·0
—— of three observations each day at 8, 2, and 8 o'clock . .	67·1

Evaporation for the month 1·95 inch
Rain near ground 3·145 inches.—Rain 23 feet high 2·925 inches.
Prevailing Wind S.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 12½; an overcast sky without rain, 10; rain, 5½.—Tot. 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr	Nimbus.
10		31	0	19		26	22

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	2	0	1	3	10	6	7	31

General

General Observations.—The first part of this month to the 16th was wet and windy, and the latter part cloudy and fine, with occasional showers of rain and hail. After so dry a winter the rain was very seasonable, and the subsequent mild sunny days brought on a sudden spring, which has accelerated the budding, and is rapidly unfolding the leaves of the trees.

Although the thermometer has only receded twice to the freezing point, and the weather mild for March, yet the spring is comparatively backward, which is a favourable circumstance, as an early spring is not very desirable in this latitude, in consequence of the night frosts which generally follow. On the 15th at 1 P.M. we had a smart shower of snow, which is later than we remember to have seen snow here: it was brought on by a change of wind from W. to N.W. About the same time of the day on the 29th, a heavy shower of hail fell, and lay on the ground a short time: this was also brought on by the inosculations of two winds, one from S.W. the other from N.W.

The mean temperature of the external air this month, is 2¼ degrees higher than the mean of March for the last eleven years.

Parhelia and coronæ have frequently appeared. In the afternoon of the 10th a parhelion is reported to have been seen at Southampton; and it was also distinctly seen by a gentleman at Hampton, near Farnham, who has described it in a letter as being of a very beautiful appearance. It was observed here the same afternoon, tinged with red, purple, and a silvery colour, and was distant from the sun's centre 22° 45′: the angular distance of the mock-sun from the true sun varies from 22½° to 23½°, according to the density and position of the vapour in which it is generated. When two or more parhelia appear at the same time, they alternately increase and wane in size and colour, in proportion to the intensity of the solar rays, and the humidity of the vapour from which they are reflected. In the evening of the 10th there was a discus halo round the moon, encircled by a large lunar corona 45 degrees in diameter.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are six *parhelia*, nine solar and two lunar halos, two brilliant meteors, one rainbow, lightning in the evening of the 29th, and 15 gales of wind, or days on which they have prevailed, namely, two from N., ten from S.W., two from W., and one from the N.W.

REMARKS.

London.—March 1, 2. Showery. 3. Cloudy. 4. Cloudy, with showers. 5. Showers: very stormy night. 6. Showery. 7. Showers, 8. Cloudy: windy. 9, 10. Fine. 11. Fine day: stormy night. 12. Fine. 13. Rainy. 14. Fine: rainy night. 15. Showers. 16. Fine morning: cloudy P.M. 17. Windy: cloudy: showers. 18. Fine: windy. 19, 20. Cloudy. 21. Rainy evening. 22—27. Fine. 28. Rainy night: high wind. 29. Cloudy: a shower of hail, P.M. 30. Some hail about noon, very bleak wind. 31. Fine.

Boston.—Mar. 1. Stormy. 2, 3. Cloudy. 4. Cloudy: stormy night with rain. 5. Fine. 6. Windy. 7. Fine: showery A.M. and P.M. 8—10. Fine. 11. Cloudy: rain A.M. 12. Fine. 13. Fine: rain P.M. 14. Stormy. 15. Cloudy. 16. Fine: rain P.M. 17. Stormy: blew a hurricane all day. 18. Stormy. 19—25. Cloudy. 26. Fine. 27. Cloudy: rain at night. 28. Stormy. 29—31. Fine.

Penzance.—Mar. 1. In general rain. 2. Fair. 3. Rain. 4. Rain: fair. 5, 6. Rain. 7. Showers. 8. Rain: fair. 9. Fair. 10, 11. Fair: showers. 12. Fair: rain. 13. Rain. 14. Clear: fair. 15. Rain: fair. 16. Clear: showers. 17. Showers: clear. 18. Clear. 19. Fair. 20. Misty: rain. 21. Fair: clear. 22. Clear. 23, 24. Fair: clear. 25. Fair: showers. 26. Fair. 27, 28. Showers: hail and rain. 29, 30. Showers. 31. Clear.

Meteor-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.		Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
		London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
		Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.										
March	1	29·52	29·45	29·20	29.20	29·47	29·39	28·85	55	48	52	50	54	45	51·5	SW.	W.	SW.	W.	...	...	0·37	...	0·690	0·25
	2	29·65	29·46	29·40	29·12	29·54	29·27	29·10	49	37	49	40	51	40	39	SW.	NW	SW.	calm	...	...	0·24	1·250	·060	...
	3	29·45	29·06	29·10	28·70	29·45	29·04	29·16	49	39	51	44	51	43	41	SW.	S.	S.	SW.	...	0·10	0·08	...	·140	...
	4	29·93	29·06	29·30	28·58	29·19	28·79	28·50	51	30	45	42	50	32	44	SW.	SW.	SW.	W.	...	...	0·08	...	·005	...
☽	5	29·93	29·28	29·50	29·14	29·73	29·39	29·10	49	39	40	34	51	45	34	SW.	W.	W.	W.	...	...	0·10	0·790	·440	·13
	6	29·74	29·28	29·10	28·94	29·14	29·03	28·70	50	34	52	42	52	38	48	SW.	W.	SW.	SW.	...	·20	0·12	0·645	·090	...
	7	29·74	29·09	29·12	28·70	29·50	28·98	29·25	52	42	50	46	57	44	40	SW.	SW.	S.	W.	...	...	0·05	0·120	·070	·12
	8	29·61	29·09	29·00	28·64	29·15	28·87	28·52	51	27	43	42	50	34	46·5	SW.	W.	SW.	W.	...	...	0·01	...	·020	·1 2
	9	29·96	29·61	29·24	29·10	29·41	29·32	29·16	46	25	45	34	44	32	34	NE.	NE.	NE.	W.	...	·20	...	...	...	...
	10	29·96	29·74	29·50	29·40	29·80	29·68	29·00	52	35	52	32	47	41	35	E.	SE.	NE.	SE.	...	...	0·13	...	·260	...
	11	29·87	29·75	29·40	29·38	29·62	29·50	29·12	56	36	53	45	56	46	49·5	W.	W.	SW.	W.	...	...	0·32	...	·210	·15
	12	30·05	29·87	29·78	29·68	29·92	29·67	29·22	55	46	54	48	56	47	46	W.	W.	W.	W.	...	·10	...	0·755	·090	...
○	13	30·05	29·95	29·74	29·70	29·92	29·74	29·44	56	44	54	50	57	44	43	NW.	W.	W.	NW.	0·47	...	0·06	0·350	·030	...
	14	29·95	29·81	29·88	29·80	29·96	29·81	29·30	49	40	50	46	54	42	46·5	NW.	W.	NW.	NW.	...	...	0·20	...	·210	·10
	15	30·32	29·81	29·84	29·60	29·87	29·64	29·24	48	29	48	45	49	35	44·5	NW.	W.	NW.	NW.	...	·15	0·06	0·340	·030	...
	16	30·32	29·56	30·00	29·98	30·15	29·97	29·80	51	42	51	40	51	45	49	NW.	NW.	NW.	NW.	...	...	0·22	...	·270	...
	17	30·21	29·55	29·76	29·58	29·68	29·43	28·95	45	32	51	48	53	35	38	NW.	NW.	N.	NW.	...	...	0·03	...	...	·28
	18	30·50	30·21	30·00	29·98	30·20	30·04	29·75	45	25	50	41	47	34	38	NW.	N.	N.	NW.	0·55	·30	...	...	...	...
	19	30·50	30·41	30·05	30·02	30·30	30·27	30·00	52	40	51	41	54	45	37	SW.	W.	SW.	SW.	...	...	...	...	·020	...
☾	20	30·41	30·24	30·08	30·06	30·25	30·21	29·85	55	47	53	48	57	47	50·5	W.	W.	SW.	W.	...	...	...	0·170	...	...
	21	30·24	30·23	30·00	30·00	30·13	30·10	29·72	54	45	54	48	57	46	47	NW.	W.	W.	W.	...	·15	0·05	...	...	...
	22	30·23	30·23	30·00	30·00	30·14	30·10	29·64	58	38	56	46	56	43	49·5	NW.	W.	NW.	W.	...	...	...	...	...	·13
	23	30·23	30·22	30·00	30·00	30·12	30·11	29·60	60	43	55	48	60	44	52	NW.	W.	W.	NW.	...	...	...	...	...	...
	24	30·22	30·16	29·94	29·90	30·10	30·08	29·58	56	37	52	47	60	46	51	SW.	W	W.	W.	...	·20	...	...	...	...
	25	30·44	30·16	29·88	29·86	30·11	30·01	29·60	40	28	53	45	58	34	44	NW.	NW.	N.	N.	...	...	...	...	...	...
	26	30·44	30·17	30·00	29·90	30·25	30·17	29·92	50	30	50	43	50	36	39	SW.	SE.	SE.	N.	...	...	...	...	...	...
●	27	30·17	29·84	29·75	29·70	29·96	29·78	29·60	53	42	54	46	57	46	43·5	SW.	W.	SW.	NW.	...	·20	...	0·900	·115	...
	28	29·84	29·40	29·54	29·00	29·70	29·26	28·76	56	36	54	45	56	39	43·5	NW.	NW.	SW.	NW.	...	...	0·34	...	·340	·44
	29	29·58	29·41	29·34	29·12	29·40	29·24	29·05	51	34	47	40	54	38	42·5	W.	NW.	W.	NW.	0·95	...	...	0·500	·055	..
	30	30·28	29·58	29·58	29·34	29·71	29·41	29·40	48	36	50	40	52	36	43	W.	NW.	NW.	NW.	...	...	..	0·255	...	...
	31	30·34	30·28	30·00	29·94	30·15	30·06	29·80	58	36	52	43	54	41	43	NNW.	N.	NW.	NW.	0·23	·35	0·04	...	...	...
Aver.:		30·50	29·06	30·08	28·58	30·30	28·79	29·32	60	25	56	32	60	32	43·3					2·20	1 95	2·50	5·265	3·145	1·72

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JUNE 1827.

LXXVIII. *Observations on the Crystalline Form of Sillimanite.* *By* W. Phillips, *F.L.S. G.S. &c.**

THIS mineral was first described by Bowen in the American Journal of Science for May 1824, and is therein said to occur in rhomboidal prisms of about 106° 30′ and 73° 30′, the inclination of the base on the axis of the prism being 113°; and as having one cleavage parallel to the longest diagonal of the prism. In the translation by Haidinger, of Mohs's System, &c. the measurements quoted are the same as those given by Bowen.

This mineral being always found, I believe, imbedded, and chiefly in quartz, its prisms are frequently somewhat bent, sometimes even twisted; and their planes being generally far from bright, and occasionally somewhat convex, are mostly unfit for accurate measurement. Having attempted to measure several by means of the reflective goniometer, I was unable to find any one affording the measurements above mentioned; but almost uniformly found the prism bounded by several longitudinal planes, of which however it was difficult to obtain the measurements, for the reasons already stated. At length I succeeded in detaching from the matrix some very thin prisms presenting but four lateral planes, which afforded constantly angles of about 88° and 92°; but they were far from bright: these slender prisms are nearly transparent and colourless, or have only a slight tinge of yellow; and at first some doubt arose whether they were in reality prisms of sillimanite. They agreed with it in being very hard; but it occurred to me that if I could procure the easy and brilliant

* Communicated by the Author.

plane of cleavage parallel to one of the diagonals of the prism, it would satisfactorily prove them to be that substance: and the attempt succeeded; but I found it to be parallel to the shortest diagonal of the prism.

Again, taking advantage of this cleavage, I obtained it on some of the larger prisms having several lateral planes: it is represented by *f* on the second of the following figures; and the measurements accompanying the figures were afterwards obtained.

M on M′	about	88°
——— *f*	. . .	134
*g*1 on *f*	. . .	145
*g*2 on *f*	. . .	152

M

M *g1* *g2* *f* *g2* *g1* M

From the foregoing circumstances I am led to believe, that a rhombic prism of 88° and 92° may, in default of better evidence, be adopted as the primary form of this mineral: the planes of this prism are brighter than the other planes. The terminations of the prism are, according to Bowen, oblique to the axis; but I have not succeeded in my attempts to find any indications of cleavage in that direction, nor at right angles to the axis. The crystals, indeed, are often separable with ease nearly in that direction, owing apparently to natural fissures across it; but the surfaces produced by the fracture are neither even nor brilliant, nor at a constant angle with the axis. Besides the brilliant cleavage already mentioned, imperfect indications of another at right angles to it may sometimes be observed.

From the close agreement in the analysis of this mineral with that of kyanite, and the measurements adopted by M. Haidinger, as may be assumed from Bowen, which also agree very nearly with those of that mineral, he is of opinion that sillimanite is probably a variety of kyanite; an opinion which it is probable will at least receive a revision, on taking into consideration the circumstances above detailed relating to form, measurement and cleavage, in all which the two minerals seem to disagree very decidedly.

LXXIX. *Re-*

LXXIX. *Remarks on* Mr. J. Taylor's *Paper on the Explosion of Steam-Boilers.* *By* AN ENGINEER.

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

I HAVE experienced much gratification in reading in your Number for February a paper by Mr. John Taylor On the explosion of steam-boilers. The public are indebted to him for bringing forward a subject of so much interest and importance; and I am glad to observe that he has set an example which I hope to see followed,—that of discussing *practical* subjects in a scientific Journal.

Having been professionally engaged in the application of high-pressure and common steam in all its branches, the subject of steam-boilers and the accidents to which they are liable have of course occupied my attention.

I have accordingly availed myself of every opportunity afforded me, of personally examining or inquiring into the circumstances which have attended such explosions of steam-boilers as happen to have come to my knowledge. The result of my inquiries has generally been, that these accidents have originated either from the carelessness or ignorance of the persons attending the boilers, or from the bad construction of the boilers themselves.

The extreme difficulty of obtaining any thing like information to be relied upon on these occasions, must have been felt by all those who have endeavoured to acquire it. It has too often happened, that the only person or persons competent to give such information, have lost their lives by the effect of the explosion; and where this lamentable result does not take place, it is too much to expect that men will confess their carelessness when a loss of situation would be the inevitable consequence. Hence it arises that, in order to divert attention from themselves and conceal their neglect, they invent a wonderful tale of the explosion being preceded or accompanied by something extraordinary; such as a flash of flame, a rumbling noise, &c. &c.—tales which I have often heard repeated. Even where there exists no intention of deceiving, the love of the marvellous (so common to the lower classes), or the agitation produced by terror, will cause them to magnify the most trivial occurrence into something out of the common.

Having expressed an opinion that these accidents are always attributable to neglect, or the originally bad construction of the boilers, I shall proceed to inquire how far one or other

of these causes has in all probability produced the several accidents alluded to and detailed by Mr. Taylor. This inquiry is rendered more simple and easy from the circumstance of the same description of boiler having been used in each of the four instances.

As I am not in a situation to *prove* that these accidents have originated in neglect, I shall only observe under this head, that as far as appearances after explosion are to be relied upon, they were such as to justify a suspicion that the boilers at Polgooth and East Crennis were short of water. I do not think it necessary to pursue this part of the inquiry further, because I am prepared to show that the construction of these boilers, and the mode of setting them, is quite sufficient to account, not only for these, but for every accident that has ever occurred with boilers of a like description.

In boilers intended for high-pressure steam, three important considerations are to be attended to: viz. the material employed, the form, and the mode of setting. With the experience we now have in these matters, no one would think of using any other material than wrought iron. The form ought to be such, that the expansive force of the steam can produce no effect tending to change the form; or in other words, that the expansive force is equally exerted over the whole of the internal surface of the boiler. This object is attained in the spherical form, or the cylinder with hemispherical ends. This is the form to which I give a decided preference; not only for the reasons above stated, but from the circumstance that notwithstanding the smallness of diameter, a great depth of water may be kept above the fire-line,—a point in my opinion of the greatest importance. The diameters should be small, never exceeding five feet, and any augmentation of capacity should be attained by increase of length; in this way also an increase of heating surface may be obtained to any extent.

The mode of setting boilers must vary according to circumstances; such as quality of fuel, &c. Care, however, should be taken to expose as much surface to the action of the fire as is consistent with the allowing of a considerable depth of water above the fire-line.

In no respect, except in the material employed, do these Cornish boilers correspond with my idea of a good and safe boiler. The right angles formed by the flat ends are subject to an immense strain, and the angle iron introduced in these parts is ill-calculated to resist it. It is well known to engineers that angle iron will not resist the same strain, substance for substance, as iron in any other form,—a fact which may be accounted for by the distortion which it undergoes during the

process

process of rolling it. Mr. Taylor admits that in theory these angular parts are bad; but he goes on to say, that "it does not appear in practice that these have been the first to give way." Whether they are the *first* parts to give way, I cannot pretend to say; it is enough for me to know that they have eventually given way, and that it is to the failure of these parts that the tremendous effects related by Mr. Taylor are to be attributed. It is not at the junction of the angle iron to the outer case where I should apprehend the greatest danger, because the outer case is not liable to a change of form. I apprehend more danger from those parts where the inner tube joins on to the front; because, as I shall presently show, the inner tube is extremely liable to a change of form; and accordingly it is here where a fracture is exhibited in all the instances above alluded to.

I consider the introduction of a tube within a high-pressure boiler to be bad under any circumstances, but it is peculiarly so where the furnace is placed in it. If these boilers had their fire-places underneath, as shown in Mr. Taylor's second sketch, and the tube used only as a return flue, some of my objections would be removed, and I conceive a better effect would be produced: a larger surface would be exposed to the direct action of the fire, and there would be as much heating surface generally. The fire-place and ash-pit could then be made of any size required; the latter of which is by necessity most objectionably small where the furnace is inside the boiler. This is an evil of some magnitude, both as regards the draught, as well as the wear and tear of fire-bars. Lastly, but not the least important consideration in a tube-boiler, the water would be equally heated throughout.

The objections raised by Mr. Taylor's Cornish agents to a brick furnace would of course apply to this mode of setting a tube-boiler, as well as to the use of a plain cylinder where the furnace is by necessity of brick. I confess the adhering of clinkers to the *sides* of a brick furnace to such an extent as to injure the draught, to be perfectly new to me; and as Mr. Taylor does not state this of his own knowledge, he must excuse me for doubting the fact.

Having stated under what circumstances the tube-boilers may be rendered less objectionable, I shall proceed to consider them as they are at present used in Cornwall, and point out what I take to be the defects, as relates to their liability to accident.

In the first place, I consider the want of space in the boiler over the fire-place a serious evil. If too much of this space be occupied by water, then there is not room enough left for steam;

steam; and the consequence is the passing over of a quantity of water into the cylinder, to the injury, and sometimes to the destruction of the engine itself. On the other hand, if this space be divided, not in depth but in cubical contents, there is great reason to apprehend the water being allowed to get below the top of the tube: a temporary derangement of the feed-pump might occasion this, without any very great neglect on the part of the engine-man. If this takes place, I scarcely need point out what must be the result. The expansive force of the steam exerted upon the plates rendered soft by the action of the fire, would bring down the upper surface; and when once the cylindrical form was lost, a further depression would be rendered easy. It is quite clear that the upper surface of the tube cannot be depressed, without such a strain being thrown upon the ends where they form a junction with the angle iron, as finally to rend them off. I should observe here, that the sort of fracture described by Mr. Taylor in the boiler at East Crennis, would in this case present itself; the angle iron would appear to be wrenched off by a force drawing it inwards.

Even where the water is not so far reduced in the boiler as to be below the top of the tube, I am by no means inclined to consider this boiler in a safe state. At all times the upper part of the tube is more expanded than the lower, in consequence of the water above being hotter than it is below; and although this may not produce an immediate effect, yet it very probably renders the parts, where the strain is thrown in consequence of this unequal expansion, more disposed to give way when a further stress comes upon them. A few inches of water over the tube would certainly prevent the plates from becoming red-hot, and perhaps the leaden plug from melting; but it would not be sufficient to prevent the strength of the iron being impaired.

The strength of iron is much impaired before it arrives at the heat of melted lead. I have every reason to believe (and this belief is founded upon effects which I have myself witnessed), that the plates of a boiler urged by an intense fire, and covered with only a thin stratum of water, become very considerably hotter than the steam and water above them. I account for it in this way:—When the column of water is diminished to a certain extent, the weight of it is not sufficient to keep it in contact with the plates, the continuous escape of steam bubbles keeping it off. This effect may be observed in an open pan placed over an intense fire, and containing a thin covering of water or other fluid; the whole appears to be a mass of bubbles, and the bottom of the pan may be occasionally

casionally seen. I find I am by no means singular in this respect; for on conversing some time since with an intelligent practical engineer on the subject, he gave it as his opinion that a boiler was not safe where there was only a foot of water over the fire. I do not go to this extent, but I mention it in corroboration of my own opinion.

With this view of the matter, I see nothing very extraordinary in the circumstance of the leaden plug remaining as happened at East Crennis; nor, I confess, does the appearance and form of the tube after the explosion surprise me more than the projection of the tube at Polgooth, which is equally unaccountable. It by no means follows, because the sides of the tube were flattened, that these were the *first* parts to give way when such an immense volume of elastic vapour was suddenly let loose: there is no accounting for the effect it may produce upon the parts in immediate contact with it; this may even go to the extent of obliterating impressions made immediately antecedent to the explosion.

If an accident take place with a boiler of any given form, I should not be guided by the appearance that the wreck may present after the explosion, as to any opinion of what were the first parts to give way; but I should examine as to whether from its form or construction the boiler contained any weak points; and taking it for granted that these must have been the first to give way, I should make the necessary alterations.

That these Cornish boilers are more liable than any others to accident, is proved from the result of Mr. Taylor's own experience. If therefore they possess the defects which I have endeavoured to point out, it is fair to infer that such accidents are attributable to these defects.

It has been observed by Mr. Taylor, that in the opinion of his Cornish agents these boilers possess advantages which no other form affords, and that in comparison with the plain cylinder in particular, more duty is effected.

They certainly ought to possess great advantages as to consumption of fuel, to compensate for the frequent accidents to which they are liable,—accidents not only involving loss of property, but too frequently loss of lives. If this latter consideration only were taken into account, there ought not to be one moment's hesitation as to their total rejection. I am, however, by no means prepared to admit that the tube-boiler is more œconomical as to fuel than the plain cylinder. Mr. Taylor observes, that in North Wales, boilers of the latter description are giving great satisfaction, while in Cornwall this by no means appears to be the case; but this he attributes to the difference of fuel. As I have before said, a difference in fuel may

may render some change in the mode of setting necessary, or even some modification of the boiler: such as a diminution in diameter and increase in length where the coal is bituminous; and the contrary, where the coal is the reverse of this: but I am quite sure that the cylindrical boiler may be so modified and set as to suit every variety of coal. The Monthly Reports may prove that more duty is effected by the tube-boilers; but the conclusion I should draw from this is, that the plain cylinder has not had a fair trial in Cornwall. Long and continued use of the former has created a strong prejudice in their favour, and it will take some time and require some management to overcome this.

In the hands of such a man as Mr. Taylor, this desirable end may, however, be accomplished; and I should venture to suggest, that in no way could he more beneficially employ the influence which he has so deservedly attained by his character and talents, than by exerting it to put an end to the use of so dangerous and destructive a machine.

Yours, &c.

AN ENGINEER.

LXXX. *Remarks on* Mr. J. Taylor's *Paper on the Accidents incident to Steam-Boilers. By Mr.* W. J. HENWOOD.

To Richard Phillips, Esq.

Sir,

IN a late number of your Journal, Mr. John Taylor has favoured us with an interesting paper On the accidents incident to steam-boilers, many of which he seems disposed to attribute to the explosion of gas in the flues. Thinking that this opinion, if generally received, may operate as an objection to steam navigation, as well as to the erection of steam-engines in manufactories,—this cause being perhaps further out of our reach than those to which such accidents are usually attributed; permit me, through the medium of your Journal, to offer a few remarks on Mr. Taylor's valuable communication.

After some observations on the comparative merits of boilers of particular constructions, Mr. T. proposes several questions; which I will endeavour to answer in the order in which they stand.

"The Pen-ŷ-fron engine had been stopped a few minutes, and the workmen had opened the fire-doors of three of the boilers, and closed the dampers of two of them. The engineman observed a gust of flame from the fire-place, which was almost

almost immediately succeeded by an explosion."—"In this case had the rush of flame from the fireplace any thing to do with the subsequent explosion?" I think there can be but little doubt that the rush of flame was in consequence of some fracture having already taken place in the boiler; probably the fissure was not at first of very considerable size, as we know that wrought iron does not break at once (as is the case with cast iron), but rends. The rent being at first small, it would have occasioned the rush: but as the fissure once made weakened the boiler, and the aperture not being sufficiently large to permit the escape of a very considerable quantity of water or steam, a moment between the gust of flame and the explosion would in all probability have elapsed. "And admitting that the steam was so far within the pressure that could by mere expansive force regularly exerted injure such a boiler,—might not the rupture be occasioned by the aid that a vacuum suddenly created might produce?" That the expansive force of the steam (30lbs on the inch) was not sufficient to injure the boiler, remains yet to be proved, as Mr. Taylor has not informed us how strong the boilers were. Admitting the possible formation of a vacuum, it might perhaps help us towards a *real* knowledge of the cause: but I am not aware of any circumstances which can have been there in action, to which the power of forming a vacuum can with any appearance of probability be ascribed.

"Does not the bursting of one boiler after another as at Polgooth, seem to indicate that exterior causes operated?—Is it possible to conceive,—supposing the pressure equal in two boilers as at Polgooth, both being connected to the same steam-pipe,—that the relative strength of the two should be so exactly the same as that what would by mere expansive force burst the one, should have the same effect upon the other?"

Mr. Taylor informs us that the plates of which the interior tubes are made are half an inch thick, and those of the outer three-eighths of an inch. Now if we suppose each boiler to be made of 200 plates, would it not be truly surprising if in 400 plates there were no two of the same strength, the thickness being the same, and (as we suppose both boilers were made at the same manufactory) the quantity similar in each? Here then we have an expression of two known quantities only; whilst if we refer the accident to the agency of an explosion of coal-gas with atmospheric air, we must take into consideration the activity of the distillatory action, the facilities of escape afforded to the gas in either boiler, the intensity of combustion in the fireplace, the influx of air, &c. which leads us into a much more complicated calculation. The evidence

 then

then appears to preponderate in favour of the idea of its explosion originating in the expansive force of the steam, which it would seem was permitted to attain too strong an elasticity.

"At the Pen-ŷ-fron engine we see that the fire-door is thrown open, and then the current of air up the flue is stopped by closing the damper: the interior is filled with atmospheric air mixed to a certain extent with coal-gas; the latter is increased by the distillatory action of the fire, until the proportion is attained which is explosive; it takes fire, producing the rush of flame which would be followed by a sudden vacuum in the tube; while the other side, pressed by the steam, gives way to this sudden impulse, and is destroyed by a force very much smaller than would be required if uniformly exerted."

What Mr. Taylor says may be very possible, with the exception of the formation of a vacuum. Motion only obtains when the resistance is inferior to the force applied, and ceases (except under particular circumstances) as soon as the two forces become equal. This then is the case in the phænomena before us: the explosion may occasion a rush of air outward through the fire-door, because the elastic force of the fluids within the tube exceeds that of the atmosphere; but as soon as that within has so expanded as to be reduced in elasticity equal to the pressure of the atmosphere, no further emission of air from within the boiler can possibly ensue. Again, supposing the possibility of a diminution in volume of the gaseous matter within the boiler, the fire-door (say $1\frac{1}{2}$ foot wide and $2\frac{1}{2}$ feet long) in such boilers would afford an aperture quite sufficient to supply (at the moment of the diminution of volume) the void. Hence then it is evident that no force at all varying from the atmospheric pressure, can under any circumstance be exerted on the part of the boiler exposed to the fire.

"By some it has been suggested that hydrogen may have been generated by the decomposition of water from leaks in the boiler."

This is not improbable in many instances: but we can about as easily admit that the gas extricated from the coal, and which is required in order to support combustion in ordinary cases, produces the explosion. We also know that the coal when thrown into the fireplace is never perfectly dry, so that hydrogen is constantly evolved if water be decomposed. If hydrogen produce explosion, such explosions are constantly occurring; and if the water be not decomposed, of course the hydrogen cannot explode. In either case it is evident it would be alike innocuous.

But I believe the water is not decomposed when the boiler leaks

leaks much; and when such defects have existed in a boiler, Mr. Taylor as well as myself has doubtless observed the escape of large quantities of steam through the stacks: of course the water in such cases does not undergo decomposition.

The sudden bursts of flame from the chimneys of steam-engines when observed at night, are in my opinion much more satisfactorily accounted for, by supposing the flame to be carried further up the flue at some times than at others, by the action of gusts of air, which always operate more or less. This is perhaps more frequently observed on the chimneys of founderies or tin-smelting houses, than on those of steam-engines; and we are very sure that in the former cases no explosions ever obtain. I am, &c.

March 10, 1827. W. J. Henwood.

☞ We have received a communication on this subject from Mr. J. Moore of Bristol, in which he states that steam-engines have often exploded on their being stopped; and that the immediate cause of explosion, in these cases, is probably an additional strain on the boiler from within, produced by the steam, which previously had a free passage, being prevented from escaping anywhere but at the safety-valve; the aperture of which, compared with the content of the cylinder, into which the steam passed before, is very small. Mr. Moore also suggests, for the purpose of obviating accidents from such a cause, the application of a large valve on the tube, adjacent to the part where the steam is prevented from passing to the engine.—Edit.

LXXXI. *On the Physical Construction of Solids and Liquids. By the Rev.* J. B. Emmett*.

THAT the particles of liquids do not touch each other, is universally allowed, because the change of volume of which all liquids are susceptible by changes of temperature, is greater than any that can result from any possible alteration in the arrangement of contiguous spheres. Besides, since the particles of which a liquid is composed are *in equilibrio* between two equal forces acting in opposite directions, they can occupy but one order of arrangement; *i. e.* as Newton proved, two right lines joining the centres of three adjacent particles, must form an angle of 60°; were they not thus balanced in the point of equilibrium of two equal and opposite forces, they could not possess their observed freedom of motion. So long, therefore, as a body is in a liquid state, there can be neither

* Communicated by the Author.

 expansion

expansion nor contraction, except such as results from the receding from, or approach to, each other of the particles, in right lines joining the centres of the adjacent particles.

With respect to solids, the case is very different; the utmost expansion is very small, compared with that of the same bodies in a liquid state. Some have supposed (Lavoisier's *Chemistry*) that the particles of solids do not touch each other. Boscovich imagined them to be separated to a distance from each other, in a point of equilibrium between their own centripetal force and the repulsion of caloric;—nearer to the particle, he supposed the force of repulsion to prevail; beyond it, that of attraction, which increases, according to his system, according to some function of the distance directly, to a certain maximum: hence, if any force be applied, tending to separate the particles, since their distance is somewhat increased, the centripetal force begins to produce sensible effects; when the particles are removed to a distance beyond that at which the force of attraction attains its maximum, they separate. Were this the true state of the case, the particles of solids must have the same freedom of motion which those of liquids possess; or in other words, there could be no solid in nature. It is also evident from sect. 12, 13, of the first volume of Newton's *Principia*, that whatever law of variation the centripetal force obeys, the particles of solids must be in contact, otherwise the observed phænomena cannot be produced.

These departments of science have received but little attention from modern chemical philosophers, except so far as the subject of crystallization is concerned; and here, systems are commonly received, which seem to be at variance with established principles of physical science. For, spherical particles are so placed together, that if the centre of a particle be joined with the centres of two adjacent ones, the lines form angles of 60° or 90°: since every variety of crystal cannot be produced by such arrangements, some particles have been supposed to be spherical; others, ellipsoids, oblate and prolate, of various degrees of eccentricity. This being supposed, a crystal cannot either expand or contract by change of temperature: for if it contract, the particles must be compressible; if it expand, it is resolved into a liquid: this system cannot account for the direction of the cleavages, nor explain why, on heating the nucleus of a crystal (as a rhomb of carbonate of lime), the acute angles are increased and the obtuse diminished. Again, since nearly all known crystals are compound bodies, this system has to suppose a compound atom; *i. e.* a system of several contiguous atoms, to assume the form of a regular ellipsoid, or some such figure, in all, except a few cases. The argument

argument which militates mainly against the hypothesis, is drawn from the phænomena of expansion and contraction, which are impossible, as it is framed at present, which is excessively nearly, and in most parts precisely, the same with Dr. Hooke's (See *Micrographia*).

In a number of the Annals of Philosophy, (but at what time I cannot exactly state, since I have not the series at hand,) I endeavoured to show how the particles of solid matter, being always in contact with each other, and obeying the laws which are known to exist, may alter their relative position, and thereby produce a change in the volume of the entire mass. The particles being always in contact with each other in certain points, their order of arrangement admits of every variety between the angles of 60° and 90°, being held *in equilibrio* by the balance of two opposite forces: hence may result every variety of form in crystals, as well as the direction of the cleavages, as also the phænomenon of the enlargement of the acute, and diminution of the obtuse angles. According to this hypothesis, the force of cohesion is produced by the actual contact of the particles of matter, which force is so greatly diminished by separation to the least distance, (Newtoni *Principia*, lib. i. sect. 12, 13,) that it is commonly said to vanish: the first separation to even the least distance destroys that force which is properly termed cohesive; and the particles are then held together by the force of the whole particle, as in liquids, as has been shown in the former papers.

Were it possible to deprive bodies of all their caloric, or to reduce them to the true zero, then the particles must be in the closest possible contact; *i. e.* the centres of three adjacent particles must occupy the angular points of an equilateral triangle: in the utmost state of solid expansion, all the angles become right angles, as in Dr. Hooke's and Dr. Wollaston's figures; between these extremes, the expansion of solids takes place. It is then easy to compute the utmost degree of expansion of which a simple solid, if such exist, is capable, as well as the distance to which the particles of a solid must separate from each other, in order that it may expand or contract during fusion. Form a rhombic parallelopipedon of small spheres, placed in rectilinear rows, and so that each sphere of one row shall be in contact with two spheres of the next. Let A be one side of the rhombus; *a* one of the acute angles; R the tabular radius; the solid content is $\frac{A^3 \times \sin^2 a}{R^2}$ if there be *n* spheres on each edge of the solid, the diameter of each is $\frac{A}{n}$, and its radius $\frac{A}{2n}$: its solid content is $\frac{p. A^3}{6n^3}$; since there

are

are n spheres on each edge, the number in the whole solid is n^3; therefore the whole solidity of the spheres is $\frac{p \cdot A^3}{6}$; and the sum of the interstices $= \frac{A^3 . \sin^2 a}{R^2} - \frac{p . A^3}{6}$; which is known when a is given.

The whole content of a cube, whose side is $A = A^3$; therefore when the particles are arranged in a square form, the sum of the interstices $= A^3 - \frac{p . A^3}{6}$. The angle a in this paper, is the angle B in the former papers here quoted. In any solid, let a become a right angle just before it fuses; its content is A^3; after fusion, let the distance A become $A + h$; the content then is $\frac{(A+h)^3 . \sin^2 60}{R^2} - \frac{(A+h)^3}{R^2} \times \frac{3}{4}$; hence, in order that there be neither expansion nor contraction during fusion, $h = A \left\{ \sqrt[3]{\frac{4}{3}} - 1 \right\}$ if h be greater, the body expands during fusion: if less, it contracts. And the utmost expansion of which a solid is susceptible $= A^3 \left\{ 1 - \frac{3}{4} \right\} = \frac{A^3}{4}$, this comprehends the whole possible range from the true zero, or total privation of heat, and results from the change which may take place in the relative position of equal spherical particles, held together and in perfect contact, by cohesion.

The particles of liquids are *in equilibrio* between two opposite forces, viz. attraction and the repulsive force of caloric. The latter, in liquids decreases more rapidly than the former; for liquids expand by heat, and contract by being cooled: but if it varied inversely as a lower power of the distance than the centripetal force, the contrary must happen; if according to the same power, matter could not exist in a liquid form, except under pressure. This is evident from the nature of forces.

Now, when a solid is heated, the angle a increases, until the force of cohesion is overcome: the particles then separate from each other, and are arranged in regular hexagons: by the evidence derived from experiment, the force of repulsion exceeds that of attraction to a certain distance, beyond which it is inferior to it; the distance of the point of equilibrium is, therefore, a measure of the distance between the particles; and this point recedes from the particles as the temperature is increased; until, by a certain increase of temperature, the particles become altogether repulsive, or the matter becomes gaseous.

J. B. EMMETT.

Great Ouseburn, March 12, 1827.

LXXXII. *On*

LXXXII. *On retaining Water in Rocks for Summer Use.* By Mr. WILLIAM SMITH, *Engineer, M.Y.P.S.**

AS practical applications of knowledge acquired from geology in relation to the comforts and conveniences of man in a most essential article of life, must be considered matter of importance, I hope to be excused for troubling the Society with a detailed account of what I may call a Geological Reservoir of water made in the hills near Scarborough in the dryest summer this country has experienced for sixty years.

We know from the annual variation of springs, that rocks hold a much greater quantity of water in winter than in summer; and we further know, that in wet seasons rocks hold periodically much more than their annual average quantity both in winter and summer: and hence the question as to the possibility of retaining water in rocks for summer use is decided by the annual and periodical operations of nature.

For the means of altering or improving some of these natural operations, so as to render the irregular supply of water which falls upon the earth more convenient to the general purposes of man, we must resort to geology;—to find what stratum is fitting for the object, and what site in the range thereof; what the rock lies upon; what stratum or diluvium covers it, and the dip, rises, and troughs or undulations in the strata.

I have for many years entertained notions of the practicability of making use of rocks as subterraneous reservoirs of water, in some cases extensive enough for the use of canals: and once, in a Report on Springs, suggested such a plan to one of our canal companies. But for the use of towns and dwelling-houses, many situations may be found where the joints of a rock are capacious enough for penning up winter water therein, for use in even the dryest summers; as many springs which then fail, produce a superabundant quantity in winter.

This was the state of the first springs anciently taken from adjacent hills to supply the town of Scarborough; which supply has been from time to time increased and improved at the expense of the Corporation. Within a few years new pipes have been laid at a great expense.

Still however, in the summer months, when there was much company in the place, water was deficient; and the commissioners for improving the town undertook to search for more water on the hill sides about a mile and half distant.

In the month of May last a small quantity was found to issue

* Read to the Yorkshire Philosophical Society, March 1827; and communicated by the Rev. W. V. Vernon, Pres. Y.P.S.

issue from a bore-hole made several years since for draining the land. On cutting an open channel up to this, the discharge increased and at the depth of nine or ten feet amounted to twenty-four hogsheads per hour. This encouraged them to proceed; and the channel under my direction was deepened four feet, when the discharge became for some time fifty or sixty hogsheads per hour.

Suspecting from an intermediate and subsequent diminution that we had drawn off a confined stock of water, and that the regular run of the spring at the end of a dry summer might not be found sufficient, I suggested the propriety of damming up the produce of this spring for summer use, as the previous supply was more than sufficient for the town in winter.

The circumstances were favourable for the purpose, as there was no other known issue of water from the rock in that hill, which is about a mile long, narrow on the top, and insulated in all the upper part of its stratification. The same rock is not opened or known any where else on these hill sides, but in a deep valley which separates the insular hill from the main and higher hill of Falsgrave Moor. In the upper end of that valley a spring was opened several years since in the same kind of rock, and was brought with a declivity of thirty or forty feet round the south end of the insulated hill, near to and high enough to run into the opening made to the new spring. This was sufficient to prove the general rise of the rock westerly in the base of the insular hill, and beneath an isthmus connected with the main ridge of Falsgrave Moor and Seamer Beacon. The rock in which the spring was found is a yellowish fine-grained crumbly sandstone, in thick beds, with open irony joints, the same as in the cliff south of Scarborough Spa. From the quantity of carbonaceous matter in it, it is here called "coaly grit." This sandstone, with its overlying and alternating clays, is analogous in position to the clay and sand and sandstone between the cornbrash and great oolite rocks. At the depth of ten feet the rock was found covered with regular clay about four feet thick; on this a mark of coal, and a thin bed of hard stone full of imperfect vegetable impressions; and up to the surface a very tenacious *slidden* clay. The rock was found, by boring through it, to be ten feet thick, lying on clay. The channel excavated up to the spring about thirty or forty yards long, and fifteen feet deep, at the upper end was entirely in a very tenacious clay partly diluvial, with a few rounded stones in it deeply covered by slidden clay. Within four feet of the edge of the rock lay gravel (deeply covered also with slidden clay), consisting of large and small boulders

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of whinstone, granite, mountain-limestone, &c. which gravel, between the clay and the face of the rock tapered downward "to nothing" in the bottom of the excavation.

About two yards within the edge of the rock (which was nearly as upright as a wall) a basin six feet in diameter and four feet deep was excavated, to receive the water flowing from the joints of the rock. Cast-iron pipes branching from the main line of pipes were laid up to this basin, to receive the regular flow of the spring, which before the end of summer was reduced to less than six hogsheads per hour. The clay channel, in the bottom of which the pipes were laid, was refilled with clay and puddled, so that no water could pass from the rock but through the pipes. The end of the last pipe was closed, and a vertical aperture made for receiving the run of the spring. No further contrivance was required for stopping the water and damming it up in the rock, than an open vertical pipe ground to fit tight into the aperture in the horizontal pipe; and this to the height of four feet was done by pieces of pipe, each a foot in length, tight-fitting one into another for the convenience of wholly or partially damming or drawing off the stored water as occasion might require; the water being allowed to run in at the top of the pipe.

After the rainy days in the beginning of November last, these short pieces of pipe were put in one after another, and found to dam up the water in the joints of the rock to the height of four feet, which from the quantity wasted last summer during the progress of the works, was calculated to contain 5000 hogsheads. The vertical pipe being since closed at top, (and lately also the main iron pipe,) the whole of the water from those parts becomes forced into the cavities of the rock, and now stands 14 feet deep at the spring, or 10 feet higher than we calculated upon penning it; so that the subterraneous reservoir may contain 12,000 or 15,000 hogsheads of water. This will be ascertained in the summer as it is drawn down from time to time into the new arched reservoir in the town. This reservoir, formed of a brick cylinder 18 feet deep, sunk in the ground, and covered by a dome 40 feet span and 20 feet high, surrounded by a strong bank of earth, is calculated to contain 4000 hogsheads.

Scarborough, Feb. 5, 1827. WILLIAM SMITH.

LXXXIII. *Outlines of a Philosophical Inquiry into the Nature and Properties of the Blood; being the Substance of three Lectures on that Subject delivered at the Gresham Institution during Michaelmas Term* 1826. *By* JOHN SPURGIN, *M.D. Fellow of the Royal College of Physicians of London, and of the Cambridge Philosophical Society.*

[Continued from p. 376.]

OUR knowledge, however, of the red globules, would be extremely limited, were it not for the assistance of the microscope; and although great discrepancy of information exists among various microscopic observers respecting them, still they all agree in regarding them as organized bodies, having different ingredients entering into their composition. Their appearance before the microscope will be stated presently. With regard to their chemical composition and chemical properties, the latest writers on this subject, and the latest compilers of the sentiments of others, observe, that these bodies still remain the subject of controversy; for although they have engaged the attention of some of the most acute modern chemists, the results obtained by them are so discordant, that no consistent or decided conclusion can be deduced from them. The greatest reliance, though by no means an implicit one, is placed on Berzelius, who spent so much time in this department of animal chemistry; and his conclusion is, that these particles do not materially differ from the other parts of the blood, except in their colour, and in the circumstance of a quantity of the red oxide of iron being found among their ashes after combustion. The presence of iron cannot be detected however, by the most delicate test, previous to the calcination; whence Dr. Bostock supposes it to exist in no form answering to any of the known salts of this metal. That this is the cause of the red colour, Dr. B. thinks may be admitted as a probable presumption: whilst Mr. Brande endeavours to prove that it cannot be so, because he found the presence of iron to be indicated as much in the colourless parts of the blood as in the globules themselves; or rather, his results tend to prove the almost entire absence of iron from the blood *. A very remarkable property of the red globules is their changing colour on being exposed to the action of the different gases. This change was observed to take place on exposing the

* For the most recent experiments on the *colouring matter* of the blood, which set at rest for ever the question, by confirming the existence of iron in the red particles of the blood, by Engelhart and Rose, see the *Edinburgh Medical and Surgical Journal for Jan.* 1827, vol. xxvii. No. 90, pp. 95, 96.

the blood to the air, some time before the component parts of the air were discovered: and Priestley fully proved that this is owing to the oxygenous part of the air alone; and that carbonic acid and azote have the contrary effect, reducing bright scarlet blood to a purple colour. And it is conjectured that this change is owing to the presence of iron, and experienced by the red globules alone.

The preservation of the life of the blood, and thence of the body, would seem greatly to depend upon the change by which this bright scarlet colour is constantly renewed and preserved; for as the blood loses this colour by its circulation through the body, it is made to pass through the lungs after its arrival at the heart before it can be distributed again from the heart to the body. Now, the structure of the lungs is such as to admit of a large quantity of air being exposed to an extensive surface of a most minute and vascular net-work, whereby the dark venous blood comes almost into contact with the air admitted into the pulmonary air-cells; the consequence of this is, an immediate change of the dark venous blood to a bright scarlet colour, or to what is commonly termed arterial blood, because such is the blood contained in the arteries. But not only does the air exert such an influence on the blood both in and out of the body, but also certain other gases and certain salts will manifest a similar effect:—among the gases, the nitrous oxide more especially; and nitre, ammonia, and common salt, among the salts.

Many discordant circumstances have also been stated respecting the appearance of these globules before the microscope; and different microscopic observers have described them in a manner that might lead one to question, whether they could have been engaged upon the same subject: for the evidence of our eye-sight, and this assisted too by the magnifying powers of an optical instrument like the microscope, ought to be relied upon, if any satisfactory evidence at all can be obtained for our guidance. But this instrument may be compared in its power to the reasoning faculty of man; and we thence need no longer be surprised, that the subjects *it* is employed upon, should, like the subjects of our reasoning powers, be differently represented and differently apprehended by different persons. Many of these anomalies have been attributed to the instrument itself, and to its modification of the rays of light as they pass through it; or to its conveying the altered modifications induced by the subject under examination itself, whereby it imparts a false impression to the eye of the observer. And certainly the instrument in question and the rational faculty are nearly allied together in these respects; for our minds are very

apt to turn the rays of truth into a wrong direction; and not only so, but to be deceived in turn by the aberrations or fallacious appearances exhibited by the surface of things.

In no instance whatever have we a more striking example of our being liable to fall into error, even with our eyes open; nay more, even with our eyes armed and guarded against all possible deception! The statements of different observers are so directly opposed to one another, that in perusing them we have felt extremely desirous to view these globules for ourselves, and to be guided in our decision concerning their form and nature by what our own eyes could discern. But as we might incur the charge of seeing what our own theory and sentiments required, and thence with seeing what no one else could, we must be content with the testimony already afforded: and by collecting all the facts that are in agreement into one heap, and not casting aside but rather keeping in view those which are not, we may, perhaps, be fortunate enough to elicit something from their conflicting testimony, that will conduct us at length to a better acquaintance with these extraordinary bodies.

Malpighi, it seems, was one of the first to employ this instrument (the microscope) to investigate the blood; and he with many others described the globules merely as globules floating in the serum and imparting to the blood its red colour. But Leeüwenhoek, the greatest microscopic observer of all, by paying very diligent attention to these bodies, professed to have discovered that these were not simple spherules, but were in fact composed of a series of globular bodies descending in regular gradations: thus each of the red particles was supposed to be made up of six colourless particles, and one of these six to be made up of six other colourless particles; so that the red particle was made up of thirty-six colourless ones. Although this account of the red globules proceeded from the very highly celebrated Leeuwenhoek, and was made the basis of many theories that were advanced by the physicians of his and the following age, particularly by the renowned Boërhaave, yet it was at length disputed: and Lancisi and Senac, and afterwards the great physiologist Haller, were among the foremost to discard the doctrine altogether, by denying such a composition to have any but an imaginary existence. Hewson, Hunter, the Abbé Torré, Monro, and Dr. Young, differ in their descriptions of these globules. The first described them as consisting of a solid centre, surrounded by a vesicle filled with a fluid, and sometimes assuming an elliptical form. Hunter never could discern this latter circumstance, nor

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does he mention the investing vesicle or the central nucleus: and what is singular, he never could discover them in some animals, as in the silkworm and lobster. He regards these bodies as liquids possessing a central attraction, which determines their figure. Torré stated them to be like flattened annular bodies, or like rings composed of a number of separate parts cemented together. To Monro, they appeared as circular flattened bodies like coins, with a dark spot in the centre, which he conceived was owing to a depression, and not to a perforation. Cavallo believes them to be simple spheres. The account recently given of these bodies by Dr. Young confirms in a degree the statement of Hewson. He remarks, that if the globules be viewed by a strong light, they will appear like simple transparent spheres; but that if we examine them by a confined and diversified light, we shall be better able to ascertain their real figure and structure. The red particles of the skate, as being larger and more distinct, are better fitted for such an examination. These are almond-shaped, and consist of an external envelope containing a central nucleus. This central nucleus is independent of the envelope; for when this latter has been removed or destroyed, the nucleus still appears to retain its original form *.

With regard to the size of the red globules, there has been as much difference of opinion as we have adduced respecting their form. The medium of the most correct observations and measurements would represent a globule to be about the 5000dth part of an inch in diameter.

The external envelope, from the most recent observations, is now believed to be either principally or entirely the colouring matter, and the central nucleus itself to be without colour. It was generally supposed that the particles were soluble in water: but Dr. Young informs us, that it is the colouring matter which is contained in the envelope that is so; and he points out a method by which the central nucleus may be procured, retaining its perfect form in water, after the red part has been dissolved.

The information we possess of the origin of these red globules is extremely vague and indefinite, whilst we appear to be entirely ignorant of the mode of their formation. An interesting account is given in the Philosophical Transactions for 1819, by Sir E. Home, of some observations that were made by Mr. Bauer on the serum of the blood. Mr. Bauer remarks, says Sir Everard, "that the globules in the blood *are* produced in the serum, I first observed in July 1817; when I ex-

* Bostock's Physiology, vol. i. p. 457.

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amined a small portion of human blood on a glass plate, to ascertain the real shape and size of the globules. I then found in one square of the micrometer (which was the 160,000dth part of a square inch) two of these globules, which were separated to a considerable distance from the rest; they were entirely disengaged from the colouring substance, and lay in pure clear serum, which covered the surface of the whole square inch of the micrometer. Having placed this particular square immediately under the focus of the microscope, I attentively examined the globules for about six or eight minutes, when I perceived two extremely minute opaque spots arising in the clear serum within the same square of the micrometer, and which seemed increasing in size. In a few minutes more I perceived five or six more such opaque spots arising and gradually increasing, and assuming the same form and appearance as the two original globules; but the moisture of the serum being nearly evaporated, I diluted it with water, when all the seven new globules, as well as the two original ones, floated in the water, and appeared of precisely the same shape and white colour; and three of the new globules were of the same size as the original ones, but the rest were smaller. When left on the glass to dry, the globules remained of the same shape and size as they were whilst floating in the serum.

"The above experiment," he proceeds to say, "I have repeated a great many times with human blood, as well as with sheep's and calves' blood; and the results have been always the same. When warm and fresh blood was used, the serum covering the surface of a 160,000dth part of a square inch, produced from 6 to 12 globules; but when the serum was diluted with water, the number of globules produced was less, and they were smaller in size.

"On the 11th of August 1817, I poured half a pint of warm sheep's blood into a glass vessel, and left it 48 hours at rest to coagulate. I then poured off the serum into another vessel, in which it remained at rest six hours; with this serum a glass tube four inches long, and 3-8ths in diameter inside, was filled to overflowing, and closed with a good cork, and covered with a bladder. The serum was as clear as water; and although I examined it very attentively, I could not see more than 15 or 20 globules in the whole extent of the tube. It was kept inverted in a glass of water. At the end of seven days, upon holding the tube between my fingers, which were tolerably warm, and examining it with a double lens of considerable magnifying power, I saw some hundreds of globules rise from the bottom and ascend in a straight line in the centre of the tube, and when arrived within about half an inch of the upper

per end, they spread in all directions, and descended close to the sides of the tube; when near the bottom they re-ascended, but more rapidly than the first time; and when held longer in the warm hand, the rapidity of the motion was much increased. In two days more I found on examination the number of globules much greater; and on the 25th of September the number of globules was such as to form a sediment at the bottom of the tube of half an inch in thickness, besides a strong coat on the inside of the tube."

A similar experiment was made on human blood by Mr. Faraday, at the Royal Institution, with similar results. Dr. Bostock informs us that the buffy coat of inflamed blood consists almost entirely of these lymph-globules as they are called; and this agrees with the discovery of Dr. Dowler, that the buffy coat contains a very large proportion of serum. Dr. B. remarks further, that after much discussion respecting the structure of the red particles, Dr. Young appears to have at length decided this point, by showing that the colour of the blood is produced by a vesicle which surrounds a colourless globule; while the still later observations of Mr. Bauer, to which may be added those of MM. Prevost and Dumas, render it probable that these central colourless globules compose the fibrin.

Prevost and Dumas regard the blood as essentially composed of serum, holding in suspension a quantity of red particles, which consist of central colourless globules inclosed in a coloured vesicle or coat. When the fluid is drawn from the vessels, the central globules, in consequence, as it may be inferred, of the loss of their envelope, are attracted together, and disposed to arrange themselves in lines or fibres, thus forming the basis of the clot or crassamentum. These fibres mechanically entangle in the net-work which they form, a quantity of the serum and of the colouring matter, which, by simple draining, or by sufficient ablution in water, may be removed from them. What we then procure is pure fibrin; this substance they therefore identify with the central globule, and the clot generally with the entire particle. They consider the colouring matter as a compound of a peculiar animal substance and the peroxide of iron. Water possesses the property of breaking down these vesicles and detaching them from their nuclei, but does not dissolve them. They state that the various re-agents act upon the albumen in the same manner as upon fibrin. According to their observations the quantity of red globules in the entire mass of the blood bears an exact ratio with the temperature of the animal; and arte-

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rial contains a greater proportion of them than venous blood.—The appearances exhibited by the blood in different diseases of the body have been minutely described by different pathologists, as well as the alteration which takes place in its physical properties: but it must be acknowledged that there is scarcely a fact that can be relied upon that would indicate any decided difference in its chemical constitution. An inference is drawn from a few experiments upon the relative composition of the blood in the different periods of life,—that as age advances, the proportion of azote increases; which is consistent with the opinion of there being more fibrin in the blood of the adult than in that of the infant. Fourcroy informs us that he found the blood of the fœtus to contain no fibrin, but a gelatinous substance in its stead.

Before chemistry arrived at its present comparative degree of perfection, the only mode of examination of the blood resorted to, was to subject it to the destructive distillation, as it is called,—which consists in exposing it to the action of heat, and thence in forcing its component parts and its ultimate elements to enter into new combinations, and to yield products altogether different from any thing that can be found in the blood in its natural state. This mode of examination is certainly highly objectionable, more especially if it leads the inquirer to conclude that the products so obtained exist as such in the blood; but the later and improved mode not only has a greater claim to the appellation of chemical analysis, but is superior to the former in one essential particular, viz. in its proving to a demonstration that the elements of the blood do form new combinations, and thence entirely new products. But it at the same time proves its own fallibility and deficiency in another respect, viz. that it affords no ground for supposing that the results of its analysis are absolutely the same with what exists in and compounds the blood in its *living* state. Compared with the other mode it may be regarded as a closer approximation to the truth, but not the truth itself. Giving it its whole scope and power, this mode informs us that the great portion of the blood, which it denominates the animal matter, in contradistinction to the salts and gaseous parts, is, as to its ultimate composition, a combination of oxygen, hydrogen, carbon and azote; whilst the various forms under which it exists, are only combinations of these ultimate elements in different proportions.

Taking the vegetable kingdom in general, this mode of analysis discovers a similar law to obtain, the only exclusion being the azote, whence, chemically speaking, the great difference

ference between animal and vegetable matter, consists in the former possessing an ingredient that the latter does not, except to a very small extent indeed.

To do justice, however, to the labours and ingenuity of the past age, we are bound to confess that but little more is known at this day, concerning the proximate elements or ingredients of the blood than at the time alluded to: and that whilst some addition has been made to the store of our knowledge on this head, some material facts known to the physiologists of that age have either been lost or overlooked; and what is worse, certain facts have been recently regarded as new discoveries and observations, which are not so. The illustrious men to whose labour and ingenuity we are now adverting more especially, are Leeuwenhoek, Lancisius, Boërhaave, Gulielmus, Malpighi, Heister, and others. To adduce an instance or two: Leeuwenhoek distinctly speaks of the globules in the serum, which Sir E. Home informs us were first seen and described by Mr. Bauer, and the existence of which Mr. Faraday afterwards confirmed. The opinion of the spherules arranging themselves into right lines and forming fibres, was entertained by Leeuwenhoek before Prevost and Dumas or Mr. Bauer were in existence. The destructive distillation of the mass of the blood, as performed by modern chemists, differs but little in its results from those of an earlier date; but as there is more accuracy in chemical manipulations now than formerly, we will adduce the result obtained by this process by Dr. Thomson. "When blood is dried by a gentle heat, water exhales from it, retaining a very small quantity of animal matter in solution, and consequently having the odour of blood. Blood dried in this manner being introduced into a retort and distilled, there comes over, first a clear watery liquor, then carbonic acid gas, and carbonate of ammonia, which crystallizes in the neck of the retort; after these products, there comes over a fluid oil, carburetted hydrogen gas, and an oily substance of the consistence of butter. By the same process, and by increasing the heat, a light smoke is emitted, which affects the eyes and nose, has the odour of prussic acid, and reddens blue vegetable colours: at a more advanced stage of the process, denser fumes arise, which on examination possess the properties of phosphoric acid."

The above facts and observations, derived from the testimony of the most celebrated chemists and physiologists, are among the most important that are considered as throwing any light on the nature of the blood. But the remarks with which we set out are amply confirmed hereby; for we may see, most plainly, that the experience afforded us by one science, as chemistry for example, is not sufficient to complete our knowledge

or doctrine concerning it. On the contrary, it is indispensable for us to be acquainted at the same time with all the organs and viscera through which it circulates, as to their functions and actions; with the principles and elements of the physical sciences, and with those pathological facts which make up our knowledge of disease, or with the effects induced on the body by disease. And not only so, but we ought likewise to keep in remembrance the vicissitudes and changes of state which are induced on the body by numberless external and internal influences, whether by climate, seasons, states of the weather; or by diversities of food, medicines or the like; or by passions of the mind, which may be either in an orderly or disorderly condition:—in short, the circuit of science in its widest range; its height and its depth, must all be searched, in order to arrive at a complete knowledge and doctrine of the blood.

It will not be difficult to discern, therefore, the reason of our comparative ignorance, and of our real uncertainty concerning its true nature; for in proportion to the limited views we take of this wonderful fluid will our means fail us of escaping from the labyrinthian windings of the subject, or of extricating ourselves from those difficulties respecting the nature of life which the materialist delights in on the one hand, and the mystic broods upon on the other.

In our next lecture we shall proceed to consider the Fluidity and Vitality of the Blood.

[To be continued.]

LXXXIV. *On the Geology of East Norfolk; with Remarks upon the Hypothesis of Mr.* Robberds, *respecting the former Level of the German Ocean. By* R. C. Taylor, *Esq. F.G.S.*

[Concluded from page 353.]

THE ground upon which the town of Yarmouth stands is decidedly alluvial. Four distinct processes contributed to its formation. The first may be traced in the accumulation of heavy materials, rolled by the action of the sea; the second in the deposit of oozy sediment from muddy waters; the third in the external covering of sand by the operations of the winds; and lastly, in the rise and decay of vegetable substances.

The wind is a more powerful agent in forming the sandy belts which defend our shores, than has been imagined. Mr. Robberds has overlooked this circumstance altogether, in speculating on the origin of the low lands between Caister and Gorleston. His arguments are, that if the sea retained the same level as when it washed up the banks across this æstuary, it would occasionally still overflow those mounds; and its

its waters would be capable of sweeping away at one time, what they may have brought at another; for "it is physically impossible that water, even in a state of the most impetuous agitation, should raise any permanent barrier against its own course."

It is no less singular than true, that in the whole circuit of our shores, wherever the substantial barriers of high lands, cliffs and rocks are wanting, except in the cases of retiring unexposed inlets, nature has substituted defences of sand, accumulated by the winds, preserved by peculiar plants, and rarely requiring the assistance of man to render them effectual.

Has Mr. Robberds never rambled by the side of the sand-hills, formed by the actions of the winds, along the coast between Winterton and Happisburgh; or witnessed the remarkable ridges of sand, provincially termed Meals, by which the harbours of Cley, Blakeney, Wells, Burnham and Brancaster, are securely defended from the fury of the northerly gales? These hills are 50 or 60 feet high; they are composed of dry sand, bound in a compact mass by the long creeping roots and fibres of the plant called marram:—*Arundo arenaria* *. To this singularly useful plant the sand-hills owe their consolidation and elevation; it has been cultivated with some care upon our coast, and the industrious Dutch are indebted to its assistance for the preservation of their islands and flat coasts.

On the western coast, where the tides attain a great elevation, the marshes of Pembrey in Carmarthenshire have four or five concentric ridges of similar hillocks, forming as perfect and permanent barriers against the sea as the art of man could execute.

The mouth of the River Ogmoor, in Glamorganshire, presents a singular appearance of desolation at the present moment, through the agency of the wind and sand. Its ancient channel is filled up for two miles; houses are rendered uninhabitable, and sand-hills are raised nearly 150 feet. The mountains which bound the harbour will check the advance of this sand-flood into the interior; otherwise it threatens to overwhelm all the lands which adjoin it; while the squalls of wind, rushing down the steep valleys, occasion eddies, which deposit the sand at an elevation apparently far beyond the reach of such an irresistible enemy.

There is no need to multiply instances; and having men-

* "One of the most valuable grasses for binding the sand of the sea-shore, and raising those banks which in Norfolk, and especially in Holland, are the chief defences of the country against the encroachments of the ocean. *Elymus arenarius*, *Carex arenaria*, and even *Festuca rubra*, contribute to the same end." Smith's English Flora, vol. i. p. 172.

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tioned these, out of many of a similar description, the comparatively insignificant height to which the sand has hitherto been drifted on Yarmouth Denes (dunes or downs), will scarcely be considered deserving further discussion. At all events it may be stated, since the fourteenth century the operation has proceeded unceasingly, and may at a future period become a formidable evil to that town. It is an historical fact, that part of the ground within the limits of the Burgh is artificially raised. The ramparts round the inside of the walls were constructed in 1663, from "those little sand-banks which the sea and easterly winds had raised on the denes."

It is a well known fact, proved before a committee of the House of Commons during the last and present session of parliament, that the chief portion of the eastern marshes is even now eighteen inches to two feet below the surface of the rivers which pass through them, and that the water is artificially kept out by embankments and draining mills. Consequently, were the operations of these to be suspended, the valleys would, even under the present circumstances as to the admission of tides, be overflowed about the same depth as the unembanked Lake of Breydon.

All the tidal waters that proceed up the various streams and diffuse themselves over Breydon Lake, must previously pass through an opening or water-way only about 150 feet wide at Yarmouth bridge; and such are the obstructions so narrow a passage and the bar present to the ingress of the tide, that an eminent engineer has recently reported that the height of high-water above Yarmouth bridge is from one to three feet lower than at the haven's mouth. The average rise of the tide throughout the year at Yarmouth bridge being only three or four feet, the absolute quantity of sea water passing into the interior is therefore very small.

Let us contemplate the effect produced, when an immensely increased volume of water pressed forward, unimpeded, through several wide openings, as in the former state of the Saxon shore. It would be contrary to all analogy to assume that these inlets ever existed upon such an exposed coast, and amidst such moveable materials, without bars at their mouths, like the deep Forths of Scotland, to which they have been improperly assimilated. Nevertheles., a large body of sea water would advance, and be forced, in proportion to the width and depth of those openings and the absence or presence of obstructions, more or less far up the valleys. The waters of wide æstuaries being impelled by the force of the tides from behind, and being restricted in their channels as they proceed by the contracting high grounds, actually attain a considerably higher

higher elevation than the open sea whence they proceed. On the contrary, in narrow entrances, like the haven of Yarmouth, the tidal waters speedily sustain a material decrease in their height; and in this instance, we have seen that the level of Lake Breydon is from one to three feet lower than the ocean, from which it is separated by an alluvial bank not half a mile across. If to the thickness of the bed of ooze be added the difference between its present surface and that of the sea at high tides, we obtain the absolute depth of water which could with any probability be contained within the æstuaries, at the earliest period, before they received any portion of their covering of marine sediment. But there appear no conclusive reasons for assigning a higher level than four or five feet above the mean height of the existing rivers, at any period of which we possess historical records.

Surely Mr. Robberds's etymology of Herringby and Herringfleet is explained to favour a given theory, and must be received with caution. One at least is an evident corruption of compound Anglo-Saxon words, and has no reference to fish, whose habits lead them to avoid shallow muddy rivers. In Domesday Book, Herringby is written *Har-ing-bei.* Herringfleet is spelt *Herl-yng-flete;* and in a subsequent record we have it *Herl-inga-flet:* the two first syllables being clearly the same as *Herling*, lately written Harling, in Shropham hundred.

About the year 901 the boundaries of the counties and hundreds were defined, and the limits of parishes and manorial jurisdictions were determined. These provincial subdivisions, and even the estates into which they were further appropriated, are carefully registered in Domesday Book. It happens, without any exception, that all the boundaries of the counties, hundreds, and local jurisdictions of this district, are the rivers which wind through the various marshy valleys. It follows, therefore, that these streams had, as early as the year 900, formed themselves channels, adapted to mark the boundaries of property; which channels have continued to our times, with little alteration, except at their immediate outlets.

They were gradually embanked, as cultivation proceeded and the value of land increased. We know that the river which divides the hundreds of Flegg and Happing was embanked previously to 1274, near the Abbey of Holm; for in that year occurred a dispute about the right of fishing from the river's banks.

One mode of estimating the comparative elevation of the waters, is distinctly furnished in the causeways or dams, which were early constructed across the æstuaries. The bridge called

ed Weybrigg, at Acle, and the great causeway connecting with it, were certainly in existence in the eleventh century; and we find that payments were made towards their *repair* in 1101, and succeeding years. This causeway is so little above the present level of the river and marshes, that even in our own times it has been repeatedly overflowed. At any rate, it establishes the negative fact, that no very important change has taken place in seven centuries at a point adjoining the broadest part of the main æstuary, and only eight miles from the sea.

It is stated in a preceding page, that to a limited extent the channels of the Yare and other rivers were wider than at present; evinced by the peaty margins and the deposit of silt in the undisturbed recesses. These circumstances are confirmatory of the reduced supply of tidal waters, and show that the streams have gradually accommodated themselves to the volume of water which they have to convey.

With regard to the arrival of the Danish fleet at Norwich, A. D. 1004, no other change is needed to explain the probability of such an event, than has been accounted for. At that early period of the art of navigation, ships were constructed of little burden and of light draught; and with the advantages of several feet of tide, there could be little hazard in attempting a navigation which even at this day is capable of admitting the smaller description of coasting vessels. Nor could there be much danger under the circumstances, in an enterprize where there was neither a hostile fleet nor army to contend against; and where, on arriving at the capital of East Anglia, the invaders found the inhabitants unprepared for defence, and eager to purchase an humiliating peace.

The *Salinæ*, mentioned in Domesday Book, were chiefly situated on the north shore of the main æstuary, within three miles of its mouth; 39 of them being at Caister, and 30 more in the two contiguous parishes. None occur in the Norwich, Beccles, or Kirkley valleys; and as it does not appear that saltworks were mentioned *after* the Confessor's time, it is probable that the north entrance commenced silting up shortly after, so as to exclude the requisite admission of sea water for such works*.

Some uncertainty prevails with respect to an open communication between the ocean and the extensive watery flat near Horsea. Mr. Robberds's map shows two points by which the sea appears to have penetrated into this flat. Local records are silent upon that head. There is no mention of saltworks

* The value of a Salina was at that time estimated at sevenpence.

upon

upon its borders, or of any other circumstances positively implying such an event. From the remotest period to which we can refer, it has been a branch of the main æstuary of the Garienis, and by this channel the drainage of the district is effected. The soil is composed chiefly of peat, rather than of ooze; the first characterizing the upper parts of a valley, the latter its mouth. Whether by the gradual external wearing away of this coast, the sea approached so near this flat as occasionally to overflow the intervening bank of sand; or whether that bank results from the abrasion of the cliffs to the north, and blocks up an ancient inlet,—there are scarcely sufficient data to determine. The existence, therefore, of those northern channels, although not improbable, must remain conjectural.

There is a mistaken quotation at p. 66, stating that, A. D. 1549, an armed pinnace was sent up the Waveney, as far as Weybread. As the place is called Waybridge in the original authority, this evidently refers to Waybridge, near Acle; that being the most important pass between Yarmouth and Norwich, near which place the rebel army was encamped. Weybread in Suffolk was far removed from the theatre of operations, independently of the physical improbability of any vessel ascending this stream, at least 40 feet above the level of the Yarmouth river, and of passing half-a-dozen water-mills, which interpose in its course. It would not have been necessary to notice this error, but for the circumstance of its being classed with proofs of the altitude of the water, as late as the sixteenth century.

No further comments are suggested by the historical evidence adduced to corroborate the physical circumstances that have previously been investigated, to sustain the theory of an extraordinary reduction in the level of the waters of our æstuaries, and by inference, in that of the surrounding seas.

The result of the foregoing inquiry is opposed to that hypothesis. This inquiry suggests views of cause and effect adequate to the admitted extent of the change, which are briefly these:—

That as long as the ocean-currents set unrestricted into these æstuaries, it was in sufficient quantity to expand over and fill them; the elevation being limited by the height of the tides at the time, and the depth by the greater or less accumulation of oozy sediment.

That there was from the remotest period, through the local causes which have been detailed, a progressive decrease in the volume of this water, and by consequence a reduction, in an equal ratio, of the power to maintain an open mouth.

That

That the same causes which finally closed the æstuary at Caister, were simultaneously operating to bar the ancient haven at Kirkley, and probably to exclude the sea from the more northerly inlets.

That as soon as the admission of the tide was limited to one narrow and obstructed inlet, the quantity thenceforward was so trifling that "many thousand acres became dry, and in time good pasturage for cattle." With the assistance of embankments, the entire level of marshes became firm land; rich vegetation covered its surface, and the rivers were restricted to their deep channels.

This is the solution of that change whose traces are yet so perceptible; a solution compatible with all the real circumstances, physical and historical, with which the subject is connected. Whilst care has been taken to divest the recital of its apparently exaggerated features, abundant range has been allowed, in accordance with physical probability, for all recorded facts and fairly inferred occurrences.

There exists nothing in the series of phænomena, displayed within the limits of these eastern valleys, that is not repeated on a tenfold scale, in the fens of Lincolnshire and Cambridgeshire. We have there the spectacle of a tract as extensive as the county of Norfolk; once an inland sea, now valuable and productive land;—subjected, in its various stages, to operations similar to those on the shores of the Garienis:—reclaimed, abandoned to the ocean, and again reclaimed;—while the efforts of nature in this earth-forming process, seconded by the labours of man, have been recorded with instructive fidelity.

Assumptions founded on the limited considerations of local operations, of obvious origin and of daily occurrence, are objectionable, because the deductions drawn from thence are seldom applicable to general principles.

The filling up an æstuary by the gradual precipitation from waters charged with alluvial mud, and the consequent exclusion of the tide from its ancient receptacles, offers no better claim on which to establish the principle of a general depression of all the seas in this quarter of our globe, than the actual elevation of several feet, through obvious volcanic agency, of the bed of the Pacific Ocean for a hundred miles parallel to the Andes, proves the general depression of the entire waters of that immense ocean.

The antediluvian shells in the margin of the Norwich valley, prove a local formation only; not the general elevation of the North Sea, subsequent to the deluge. As well might Mr. Robberds have fixed the general elevation of the mighty water

waters at that point on the Apennines, where are deposited the *Buccinæ*, the *Turbines*, and *Murices*, of which analogous genera are so abundant at Bramerton: or have ascended the scale, and carried its limits yet higher,—where, at the height of twelve hundred toises, upon the Andes, M. Humboldt discovered the fossil teeth of the mastodon, whose remains are also found with the crag shells in our humble valley of Norwich.

It was the common error of the English geologists of the last century, to deduce consequences from too limited premises. Thus our Whitehurst, Woodward, Whiston, Hutton, and other intelligent observers, had each his favourite theory: each saw in the phænomena around him sufficient confirmation of a preconceived hypothesis; each reasoned and speculated from the confined data which came under his own particular observation: and, as it happens in all those cases where research is limited to the evidence of peculiar systems, the facts were not always recorded so impartially as the strictness of geological inquiry demands. Thus, for a time, schools were instituted, the disciples of which saw only through the eyes of their respective masters, and rejected truths which harmonized not with their views. It is obvious that such a process tended rather to confuse, than to simplify and facilitate the progress of this science.

This disposition to theorize has happily decreased, as the number of observers has augmented; while all unite to collect the data, to arrange the documents, and to combine those proofs, whence hereafter will arise some incontrovertible and universally acknowledged principle, by which to account for phænomena at present so inexplicable.

LXXXV. *A Synopsis of the Birds discovered in Mexico by* W. Bullock, *F.L.S. and H.S., and* Mr. William Bullock, *jun. By* William Swainson, *Esq. F.R.S. F.L.S. &c.*

[Concluded from p. 369.]

Fam. Sylviadæ.

G. Trichas. *Swains. in Zool. Journ.* No. 10.

37. *Trichas personatus.* Sylvia trichas, Wilson i. pl. 6. f. 1. Near Vera Cruz.

G. Sylvicola. *Swains. in Zool. Journ.* No. 10.

38. *Sylvicola pusilla.* Wilson iv. pl. 28. f. 1.

39. *Sylvicola Blackburnia.* Wilson iii. pl. 23. f. 3.

40. ——— *citrinella.* Wilson ii. pl. 15. f. 5.

41. ——— *flavicollis.* Wilson ii. pl. 13. f. 6.

42. ——— *inornata.*

Above olive green, beneath white; sides of the head, ears, and throat cinereous; wings with two pale yellow bands.

This, and all the foregoing species, were collected near Vera Cruz, and seem to be young birds.

G. Vermivora. Wilson. *Swains. in Zool. Journ.* No. 10.

43. *Vermivora solitaria.* Wilson ii. pl. 15. f. 4.

Inhabits with the last.

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44. *Vireo olivacea.* *Sw.* Wilson ii. pl. 12. f. 2.

Fam. Fringillidæ.

45. *Alauda cornuta.* Wilson i. pl. 5. f. 4.

To continue the specific name of *Alpestris* to a bird which, as Wilson affirms, is only seen upon sandy plains, is a manifest absurdity. I have, therefore, adopted the alteration which that accurate observer himself has suggested.

44. *Pipilo macronyx.*

Olive, head and throat black, body white, sides and vent ferruginous; wings and lateral tail feathers (in one sex) with yellow spots.

Table land. Real del Monte. Temiscaltipec.

Total length, 9 inches: wings, $3\frac{1}{2}$; tail, 4 [illegible]; tarsi, 1 [illegible]; hind toe and claw, [illegible].

45. *Pipilo maculata.*

Olivaceous brown; head and throat black; body white; sides and vent rufous; back, wings, and lateral tail feathers with white spots.

Table land. Real del Monte.

Total length, $8\frac{1}{2}$: wings, $3\frac{1}{2}$; tail, 4; tarsi, 1 [illegible]; hind toe and claw, [illegible].

46. *Pipilo fusca.*

Gray, beneath paler; throat obscure fulvous with brown spots; vent ferruginous.

Table land. Temiscaltipec.

Total length, 8: bill, $\frac{7}{10}$; wings, $3\frac{1}{2}$; tail, 4; tarsi, $\frac{9}{10}$; hind toe and claw, $\frac{7}{10}$.

The two preceding species are typical; the next is aberrant.

47. *Pipilo rufescens.*

Rufous brown, beneath whitish; crown rufous; ears grayish; chin with a lateral black stripe.

Table land. Temiscaltipec.

Total length, 7: bill, [illegible]; wings, 3; tail, 3 [illegible]; tarsi, [illegible].

G. Am-

G. Ammodramus. *Swains. in Zool. Journ.* No. 10.

48. *Ammodramus bimaculatus.*

Above gray, varied with chesnut lines and black spots; beneath ochraceous white, unspotted; breast with a lateral black spot.

Table land. Temiscaltipec.

Total length, $4\frac{1}{2}$: bill, $\frac{1}{2}$; wings, $2\frac{1}{10}$; tail, $1\frac{1}{2}$; tarsi, $\frac{3}{4}$; hind claw and toe, $\frac{5}{8}$.

G. Chondestes. *Swains. in Zool. Journ.* No. 10.

49. *Chondestes strigatus**.

Fulvous brown, beneath whitish; ears and double stripe on the head chesnut; chin with a lateral black stripe; lateral tail feathers black, tipt with white.

Table land. Temiscaltipec.

Total length, $6\frac{1}{2}$: bill, $\frac{1}{2}$; wings, $3\frac{1}{2}$; tail, $3\frac{1}{4}$; tarsi, $\frac{3}{4}$.

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50. *Fringilla socialis.* Wilson ii. pl. 15. f. 5.

Table land. Temiscaltipec. Real del Monte.

51. *Fringilla cinerea.*

Cinereous, beneath whitish; back and wing covers rufous; tail divaricated, the outer feather white.

Table land. Temiscaltipec.

Total length, $6\frac{1}{4}$: bill, $\frac{1}{2}$; wings, $2\frac{1}{4}$; tail, 3; tarsi, $\frac{8}{10}$.

52. *Pyrrhula frontalis.* Bonaparte, Am. Orn. i. pl. 6. f. 1. 2.

Table land. Temiscaltipec. Real del Monte.

Total length, $5\frac{1}{2}$: bill, $\frac{4}{10}$; wings, $3\frac{2}{10}$; tail, $2\frac{1}{2}$; tarsi, $\frac{7}{10}$.

I adopt the name by which that enlightened ornithologist Prince Charles Bonaparte has distinguished this species; although I am at present unprepared to offer any opinion as to its true affinities.

53. *Carduelis Mexicanus.*

Glossy black, beneath yellow; base of the quills and lateral tail feathers white.

Table land. Temiscaltipec. Real del Monte.

Total length, $4\frac{1}{4}$: bill $\frac{4}{10}$; wings, $2\frac{1}{2}$; tail, 2; tarsi, $\frac{1}{2}$.

Fam. Sturnidæ.

54. G. Dolichonyx. *Swains. in Zool. Journ.* No. 10.

Dolichonyx orzivorus. *Sw.* Wilson ii. pl. 12. f. 1, 2.

Table land.

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* Since the above was written, I have been gratified by a sight of the valuable addition made to "American Ornithology" by Prince Charles Bonaparte. I have it not in my power, at this moment, to institute a comparison between the bird above described, and the *Fringilla grammaca* of this writer. They appear, however, to belong to the same group; but as the characters of the genus *Spiza* are not there detailed, I know not whether it accords with my definition of *Chondestes*.

55. *Agelaus pecoris.* *Sw.* Wilson ii. pl. 18.
Table land, near Mexico.

56. *Agelaus Phœniceus.* Vieil., Wilson iv. pl. 30. f. 1, 2.
Sides of the Cordilleras. Real del Monte.

57 *Agelaus longipes.*
Blackish brown; front, temples, and throat fulvous yellow; bill short:
Table land: rare.
Male. Total length, $8\frac{1}{2}$: bill, [illegible]; wings, $5\frac{1}{4}$; tarsi, $1\frac{2}{10}$; middle toe and claw, $1\frac{1}{10}$.
Taking *L. Suchii* as the type of the genus *Leïstes*, it appears to me that the three foregoing birds, with several others in my possession, are sufficiently distinct, as a group, to remain under their former designation. So far as my information goes, *Leïstes* is a genus peculiar to South America, and is immediately connected to *Xanthornus. Agelaus*, on the contrary, is more closely allied to *Sturnella.*

58. *Sturnella magna.* Wilson iii. pl. 19. f. 2.
Table land: very common at Real del Monte.

59. *Xanthornus Baltimore.* Wilson i. pl. 1. f. 3.
Table land. Real del Monte.

60. *Xanthornus Bullockii.*
Black; rump and under parts golden yellow; lesser wing covers white; throat with a black stripe; ears and eye-stripe golden.
Table land: rare.
This, the most beautiful of the group yet discovered in Mexico, will record the name of those ornithologists who have thrown so much light on the birds of that country.

G. Cassiculus. *Swains. in Zool. Journ.* No. 10.

61. *Cassiculus coronatus.*
Black; wing covers, rump, vent, and lateral tail feathers yellow; crest elongated, pendulous; bill white.
Table land. Temiscaltipec.
Total length, 12 inches: bill, $1\frac{6}{10}$; wings, 6; tail, $5\frac{1}{2}$; tarsi, $1\frac{1}{4}$.

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62. *Icterus Dominicensis.* Pl. Enl. pl. 5. f. 1.
Table land. Temiscaltipec, not uncommon.

63. *Icterus Mexicanus.* Leach Zool. Misc. tab. ii. ex. syn.
Table land. Temiscaltipec.

64. *Icterus cucullatus.*
Golden yellow; middle of the back, front, throat, wings and tail black; wing covers with white bands.
Table land. Temiscaltipec.
Total length, 8: bill, $\frac{9}{10}$; wings, $3\frac{1}{4}$; tail, $3\frac{1}{2}$; tarsi, [illegible].

65. *Scaphidurus*

65\. *Scaphidurus palustris.*

Glossy blue black; thighs brown; bill slender, commissure straight; legs slender, claws long, slightly curved.

Inhabits the marshes and borders of the lakes round Mexico, in flocks.

Total length, 15 inches: bill, $1\frac{2}{10}$; wings, $6\frac{1}{2}$; tail, $7\frac{1}{2}$; tarsi, $1\frac{3}{4}$.

M. Vieillot's name for this group, *Quiscalus*, being already used in botany, I propose to call it *Scaphidurus*, as expressive of the singular boat-shaped tail common to most, if not all, of the species.

Fam. Corvidæ.

66\. *Garrulus sordidus.*

Blue, beneath grayish white; tail rounded.

Table land. Real del Monte.

Total length, 11 inches: bill, $1\frac{1}{4}$; wings, $6\frac{1}{2}$; tail, $6\frac{1}{2}$; tarsi, $1\frac{1}{2}$.

67\. *Garrulus coronatus.*

Crested; blue, sides of the head blackish; chin, front, and eye-brows whitish; wing covers and tertials banded with black lines; tail rounded.

This elegant bird, remarkable for its full and lengthened crest, occurs in various parts of the Table land.

Total length, 11: bill, $1\frac{1}{4}$; wings, $5\frac{1}{2}$; tail, $5\frac{1}{4}$; tarsi, $1\frac{1}{2}$.

68\. *Pica formosa.*

Cinereous gray, beneath white; crown and pectoral band black; head with a long crest of black recurved feathers.

Table land. Temiscaltipec.

Total length, $19\frac{1}{2}$: bill, $1\frac{1}{2}$; crest, 3; wings, 7; tail, 12; shortest feather, 6; tarsi, $\frac{5}{8}$.

Fam. Loxiadæ.

G. Spermagra. *Swains. in Zool. Journ.* No. 10.

69\. *Spermagra erythrocephala.*

Sub-crested; olive green, beneath yellow; head, ears, and chin, red.

To this curious bird, Mr. William Bullock has attached the following note. "Found round Temiscaltipec. Feeds on insects, but is fond of beef, &c. Two were shot on the meat at the back of my house."

The forms among the Tanagers are already so numerous, that I am not willing to increase their definitions, or rather add to the number of their genera, without due precaution. But for this, the bird before me presents such a combination of characters, that it might fairly claim a distinct station. The rounded form of the wings and tail, with the strength and thickness of the bill, associates it with *Spermagra*; but the peculiar form of the last organ brings it close to the confines of *Pyranga*, notwithstanding that the commissure,

missure, although curved, is without any appearance of a tooth.

Total length, 6 : bill, $\frac{1}{2}$; wings, 3; tail, $3\frac{1}{4}$; tarsi, $\frac{7}{10}$.

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70. *Pyranga livida.*

Livid red, beneath brighter; bill sinuated at the base; tail divaricated, the sides rounded.

Table land. Real del Monte.

Total length, 8 : bill, $\frac{9}{10}$; wings, $3\frac{9}{10}$; tail, $3\frac{1}{2}$; tarsi, $\frac{3}{4}$.

71. *Pyranga hepatica.*

Grayish livid, beneath bright red; bill toothed in the middle; tail even.

Table land. Real del Monte. The female is olive green above, and yellow beneath.

Total length, 8 : bill, $\frac{3}{4}$; wings, 4; tail, $3\frac{1}{2}$; tarsi, $\frac{3}{4}$.

72. *Pyranga bidentata.*

Head, neck, and under parts golden; back, rump and tail covers fulvous brown, striped with black; wings black, the covers varied with fulvous and white.

Temiscaltipec: rare.

Total length about 8 : bill, $\frac{3}{4}$; wings, $3\frac{1}{2}$; bill with two small teeth near the base.

G. Tiaris. *Swains. in Zool. Journ.* No. 10.

73. *Tiaris pusillus.*

Olive; crown, ears, throat and breast blackish; eye-stripe and chin golden yellow.

Table land. Temiscaltipec. Real del Monte.

A variety, or probably a young bird of this species now before me, differs in having the black confined to a narrow margin bordering the yellow spots.

G. Guiraca. *Swains. in Zool. Journ.* No. 10.

74. *Guiraca cærulea.* Wilson iii. pl. 24.

Although rarely seen in the United States, this bird is very common on the Table land of Mexico.

75. *Guiraca melanocephala.*

Head black, throat, breast, and rump ferruginous. Middle of the body, and under wing covers yellow.

Table land. Temiscaltipec. Size of the last.

76. *Guiraca ludoviciana.* Sw. Vieil. Gal. pl. 58.

Table land. I have seen two or three specimens from Mexico, but all in immature plumage.

Fam. Psittacidæ.

77. *Psittacus leucorhynchus.* Sw.

Green; crown, chin, and naked orbits white; head bluish; tail short, the lateral feathers red, margined with blue.

A pair of these birds, male and female, were brought to this country

country alive by Mr. Bullock, who purchased them in the city of Mexico.

Size of *Ps. menstruus.* Linn.

78. *Macrocercus militaris.* Edwards, pl. 313.

Table land. Temiscaltipec.

The minute description of Edwards enables me to state some few variations which present themselves in the Mexican specimen. The size is somewhat larger, being 28 inches: the two middle tail feathers alone are red, the others being blue, margined with dull red about half their length: the outer one being entirely blue.

Total length 28 inches: wings, 14; tail, 17; outer feather, $6\frac{1}{2}$. Depth of both mandibles, $2\frac{1}{2}$.

79. *Macrocercus pachyrhynchus.*

Green; front, eye-brows, and ridge of the shoulders red; cheeks plumed; tail feathers broad and obtuse.

Table land: rare.

This bird has strong claims to generic distinction; but I place it, for the present, on the confines of the true Mackaws.

Wings, 10 inches: middle tail feathers, $5\frac{1}{2}$; curve of the upper mandible, 2 inches; depth of the under, 1 inch.

Fam. Picidæ.

80. *Picus formicivorus.*

Glossy blue-black; hind head red; front, rump, and band on the quills white; throat yellow; breast black with white stripes.

Table land: not uncommon in the pine woods of Temiscaltipec. Information derived from Mr. Jenkens, a medical gentleman now at Real del Monte, enables me to give this species a name appropriate to its habits.

Total length, 8: bill, $1\frac{1}{10}$; wings, $5\frac{3}{4}$; tail, $3\frac{1}{2}$.

81. *Picus elegans.*

Equally banded with black and white; beneath gray; eye-brows black, crown red, hind head golden.

Maritime land.

Total length, $8\frac{1}{2}$: bill, $1\frac{1}{2}$; wings, 5; tail, $3\frac{1}{4}$.

82. *Picus albifrons.*

Above blackish, transversely marked with white lines, beneath olivaceous; front, chin, and sides of the head white; crown and neck red.

Table land: rare.

Total length, $10\frac{1}{2}$: bill, $1\frac{6}{10}$; wings, 5; tail, 4.

83. *Picus varius.* Wilson, Am. Orn. i. pl. 9. f. 2. Bonap. Am. Orn. i. pl. 8. f. 1, 2.

Table land. Temiscaltipec. Real del Monte.

Total length, 7: bill, $1\frac{1}{10}$; wings, $4\frac{1}{4}$; tail, $3\frac{1}{4}$.

G. Colaptes.

G. Colaptes. *Swains. in Zool. Journ.* No. 10.

84. *Colaptes Mexicanus.*

Vinaceous gray; banded above, and spotted beneath with black; throat cinereous; shaft of the quill and tail feathers bright red.

Table land. Temiscaltipec. Real del Monte. The male has a red stripe on each side the head.

Total length, $11\frac{1}{2}$: bill, $1\frac{1}{2}$; wings, $6\frac{1}{4}$; tail, $4\frac{1}{2}$.

G. Xiphorhynchus. *Swains. in Zool. Journ.* No. 10.

85. *Xiphorhynchus leucogaster.*

Chin and fore part of the throat white, immaculate; feathers of the head, neck, and breast whitish, margined with black; bill one inch and a half long, slender, pale, the upper mandible brown.

Table land. Temiscaltipec.

Total length, $8\frac{3}{4}$: bill, $1\frac{1}{2}$; wings, $4\frac{8}{10}$; tail, $4\frac{1}{2}$.

86. *Xiphorhynchus flavigaster.*

Chin fulvous white, immaculate; head, neck, and back striped with fulvous; bill long, strong, brown, slightly curved.

Table land. Temiscaltipec.

Total length, $9\frac{1}{4}$: bill, $1\frac{9}{10}$; wings, $4\frac{4}{10}$; tail, $4\frac{1}{4}$.

G. Oxyglossus. *Swains. in Zool. Journ.* No. 10.

87. *Oxyglossus maculatus.* *Sw.* Wilson iii. pl. 19. f. 3.

Maritime lands, near Vera Cruz.

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88. *Sitta carolinensis.*

Table land. Real del Monte.

Fam. Cuculidæ.

89. *Cuculus Mexicanus.*

Rufous beneath cinereous; throat and breast cinnamon; tail long cuneated, beneath rufous.

Table land. Temiscaltipec.

Closely resembles *C. cayenensis*, L., but the tail beneath is rufous, not black; the ferruginous colour of the head and neck is likewise much brighter.

Bill, $1\frac{1}{2}$; wings, 6; tail, $13\frac{1}{2}$; outer feather, $6\frac{1}{2}$.

90. *Crotophaga sulcirostris.*

Black, glossed with green and violet; bill carinated, the sides marked by transverse grooves.

Table land. Temiscaltipec. Size of the Lesser Ani.

91. *Trogon Mexicanus.*

(Female).

Ferruginous-brown; breast and body beneath red; middle tail feathers ferruginous, the rest black, the three outer pair tipt and banded on their exterior shafts with white.

Temiscaltipec.

Fam.

Fam. Trochilidæ.

G. Trochilus *Auctorum.*

92. *Trochilus fulgens.*

Green, beneath blackish, front and crown sapphire blue, upper part of the throat and ears emerald green; tail even.
Table land. Temiscaltipec.
Total length, $5\frac{1}{4}$: bill, $1\frac{1}{4}$; wings, $2\frac{3}{4}$; tail, $1\frac{3}{4}$.

93. *Trochilus thalassinus.*

Green, spot behind the ears sapphire blue; chin bluish; tail even, shining sea-green, with a broad chalybeate band.
Table land? Temiscaltipec.

94. *Trochilus melanotus.*

Golden green; front and chin sapphire blue; throat emerald green; ears black margined above with white; bill red; tail even.
Table land. Temiscaltipec. Real del Monte.
Total length, 4: bill, $\frac{7}{10}$; wings, $2\frac{2}{10}$; tail, $1\frac{2}{10}$.

95. *Trochilus platycercus.*

Green, beneath whitish; chin and throat amethystine red; tail rounded, the four middle feathers very broad: the tips obtusely pointed.

G. Cynanthus. *Swains. in Zool. Journ.* No. 10.

96. *Cynanthus latirostris.*

Green, beneath bluish; chin and throat sapphire blue; tail moderate, slightly forked, bluish black; base of the bill depressed, red.
Table land?
Total length, $3\frac{1}{2}$: bill, 1; wings, $2\frac{2}{10}$; tail (outer feathers) $1\frac{2}{10}$.

97. *Cynanthus bifurcatus.*

Golden green, beneath white, head brownish; tail rather lengthened, black, doubly forked; the six middle feathers with green tips, the two outer white with a black base; bill slightly curved.
Table land?
Bill slightly curved, base broad. This is an aberrant species, touching the genus *Phæthornis*, Sw., or that group of which *Troch. superciliosus* of Authors forms the type.
Total length, $4\frac{1}{10}$: bill, $\frac{7}{10}$; wings, $2\frac{3}{10}$; longest tail feather, $1\frac{7}{10}$.

98. *Cynanthus minimus.*

Brown, glossed with green, beneath whitish; tail short, forked, narrow and black; bill very straight.
Table land.
Total length, $2\frac{1}{2}$: bill, $\frac{4}{10}$; wings, $1\frac{1}{2}$; tail, $\frac{3}{4}$.

99. *Cynanthus Lucifer.*

Golden green; throat amethystine; the feathers elongated and narrow; tail short, the feathers pointed; bill curved.

Table land. Temiscaltipec.

This is an aberrant species; allied, by its curved bill, to *Cy. bifurcatus.*

G. Lampornis. *Swains. in Zool. Journ.* No. 10.

100. *Lampornis amethystinus.* Sw.

Green; chin and upper part of the throat amethystine; ears black, margined above with white; tail black. Female?

Table land. Temiscaltipec. Real del Monte.

Total length, 5: bill, 1; wings, $2\frac{7}{10}$; tail, $1\frac{1}{2}$.

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101. *Momotus Mexicanus.*

Head and neck cinnamoneous; back and wings green; ear feathers lengthened, black tipt with blue; beneath the eye a cærulean spot; under plumage greenish white.

Temiscaltipec.

Much smaller than the Brazilian species: on the throat are two small tufts of black feathers, longer than the others; a character which is not, however, peculiar to this species.

LXXXVI. *On some Passages in* Mr. Ivory's *Remarks on a Memoir by* M. Poisson *relating to the Attraction of Spheroids. By* G. B. Airy, *Esq. A.M., Lucasian Professor of Mathematics in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

In a paper printed in the last Number of the Philosophical Magazine and Annals, Mr. Ivory has coupled my name with terms which have never before appeared in the pages of your Magazine, or (I will venture to say) in those of any other scientific Journal. After such an attack, I am entitled to ask that you will insert in your next Number my answer to the accusation which Mr. Ivory has brought against me in so undisguised a manner.

When I read this article, I was grieved to think that I had been the cause (I think I need not say the unintentional cause) of irritating Mr. Ivory's feelings to such a degree, as to occasion the use of the opprobrious epithets alluded to. Though conscious that I had used no language, except that of courtesy towards Mr. Ivory, I referred immediately to the note to my paper in the Philosophical Transactions, of which he complains so bitterly. In it I found nothing which could justify the torrent of spleen that Mr. Ivory has vented against me. And I profess that I have said nothing in that note which

which I would not willingly hear from any one, as far below me, in all respects, as I am below Mr. Ivory.

But that your readers may judge of the provocation that I have given, I will lay before them the article which is made the ground of animadversion. The note to my paper in the Philosophical Transactions, of which Mr. Ivory complains, is as follows:

"I have not considered the second condition of equilibrium given by Mr. Ivory in the Philosophical Transactions for 1824, as the reasoning upon which that gentleman has founded the necessity of such a condition, appears to me altogether defective."

Upon which Mr. Ivory (Phil. Mag. and Annals, N.S. vol. i. p. 329, note) has made the following remarks:

"In the Phil. Trans. 1826, p. 557, there is a note of Mr. Airy, very injurious to me. He is treating of spheroids of variable density, and evidently misapprehends my conditions of equilibrium, which I have always limited to the case of homogeneity. The R. S. are not responsible for the accuracy of what they publish; but I apprehend few instances will be found so injurious to an individual, cast upon the public on the authority of mere assertion, and arising from mistaken notions. But I console myself because I know with the certainty of demonstration, that Mr. Airy's problem, admitting that any practical utility could be attached to it, is not solved, and that it cannot possibly be solved except by my theory, and indirectly, with the help of that law with which he so flippantly finds fault. What a difference between the supercilious importance of the Cambridge Professor, and the candid expositions of M. Poisson!"

I will omit mention, for the moment, of those sentences in which Mr. Ivory says that I am mistaken on the mathematical points, and will allude at present only to those in which he attacks my character as a gentleman. I will therefore state, that in my paper in the Phil. Trans., it was my business not to *investigate* conditions of equilibrium, but to make use of those already known. The equations which are best known are the one (or rather the two) commonly used, and that which Mr. Ivory has suggested. For the latter I saw no foundation, and I contented myself with a simple statement to that effect: the object and the limits of my paper not allowing me to enter into details. But, I should not have made even this statement, did I not think that the character of Mr. Ivory demanded it. I could mention the name of another writer who has added one to the common equations, but whose character did not seem to require the same compliment

 which

which I paid to Mr. Ivory's. This is merely to account for the introduction of the note. What foundation Mr. Ivory can find for the charges of "injury on the authority of mere assertion," "flippancy," and "supercilious importance," I cannot imagine. I have simply stated my difference of opinion from Mr. Ivory on a point which I was unable, from the nature of the paper, to explain at greater length. The note is now before your readers; and I appeal to them whether I have said any thing which can justify the use of such expressions. Upon the whole, I think that I have reason to complain of the terms in which Mr. Ivory has mentioned me, as most improper, and most unworthy of the respect which a gentleman ought to have for himself, as well as for any other who claims that title.

The only probable cause for Mr. Ivory's anger, independent of our difference of opinion, appears to be my omission of the reasons for that difference of opinion. The cause of that omission I have explained: but that Mr. Ivory may have for the future no ground of complaint, I shall state here my reasons for disagreeing with him. Mr. Ivory's opinion was first published in the Phil. Trans. for 1824, p. 101—108; but he has explained it in nearly the same terms in the Philosophical Magazine for July 1826. I shall request your readers, therefore, to refer to page 4 of that Number; and I shall begin my remarks at line 25. By the common theory it is known that if the forces which act on a fluid satisfy a certain equation, any level surface (*couche de niveau*) may, by the removal of a part of the fluid, become the external surface of the remaining fluid which is still in equilibrium. But this is true as a general proposition only when the forces are expressed by the same functions of the coordinates, whether the quantity of fluid be great or small. It appears then, from the common theory of fluids, that Mr. Ivory's proposition advanced in the sentence beginning in line 25, is certainly true, if there be no mutual attraction of the particles; but is not certainly true, if there be such attraction. It may happen, and in the particular case of which he treats it does happen, to be true, when the mutual attraction is taken into account, but this is quite accidental. The two following sentences are elucidations of the preceding: the latter of them is of course to be taken with the same restrictions as that of which I have treated; namely, it is to be supposed that the particles have no mutual attraction. With this supposition the reasoning of the next sentence, which depends entirely on the existence of attraction, falls to the ground. And after much consideration I am quite unable to see any force in the reasoning upon which I have commented.

Whatever

Whatever may be the meaning of the expression "similar forces," I am quite unable to discover in the sentence beginning at line 22, any grounds for the inference in line 25. Perhaps I may place the question in a clearer point of view, in the following manner. If a fluid mass in equilibrium, acted on by any external forces, and by the mutual attraction of its particles, were inclosed in a thin shell of the same shape, there would be no pressure on the shell. Or, if a pressure were communicated to the fluid (by slightly contracting the shell, suppose, or by a force acting on a small piston), the pressure on a unit of surface would be the same in every part of the shell. Now suppose some more of the fluid to be spread on the shell, and (from the action of the external forces, the attraction of the inclosed fluid, and the mutual attraction of its own particles) to receive the form of equilibrium. I do not see the slightest reason to believe that the pressure on the shell, produced by this superincumbent matter, would be every where equal. Though the whole force which acted on every particle in the original external surface must have been perpendicular to that surface, and consequently the whole force arising from the external forces and the attraction of the original fluid acts in a direction perpendicular to the shell upon the exterior particles in contact with the shell: yet there is another force not considered; namely, the attraction of the new stratum on its own lowest particles; and if this can be resolved into a perpendicular and a tangential force, the pressure on different parts of the shell *must* be unequal (from the property of equal transmission of pressure in all directions). Yet the whole fluid would still be in equilibrium, without owing its equilibrium to the existence of the shell, if the variations of the internal pressure on the shell, produced by the attraction of the external fluid on the internal, corresponded exactly to the variations of the external pressure.

Now I need not point out to Mr. Ivory that this is the case when the equation of integrability is satisfied; which holds with all the forces with which we have to do. The fluid therefore may be in equilibrium, and yet the surface which was the external surface may, for all that we can discover, be a surface of unequal pressure; and if this be admitted, the question is ended. I may remark, that even if Mr. Ivory had proved every thing which he has stated as far as line 41, the inference in the next sentence would have been unjust. "If the action of the exterior stratum does not disturb the equilibrium of the interior fluid body, this can happen only because the resultant of the attractions of the exterior matter upon any particle within the stratum is evanescent." It will be enough to

to remind Mr. Ivory, that the equilibrium would not be disturbed if the resultant of these attractions were a force expressed by a function of the coordinates of the attracted point, similar to the function expressing the previously-acting forces (including the attraction), and that there does not therefore appear to be any reason for saying that it must be evanescent.

These are my reasons for not admitting Mr. Ivory's new equation. I have stated them plainly, but I hope not uncivilly: if I am wrong, I shall be glad to have my errors pointed out in the same manner. I trust that I shall not be exposed to the charge of presumption for holding the opinion of Laplace and Poisson, in opposition to that of which Mr. Ivory is (I believe) the sole advocate.

But Mr. Ivory says that I misapprehend his conditions, which he has always limited to the case of homogeneity. When I wrote the note in question, I was perfectly aware that the algebraical investigations which Mr. Ivory had founded on his equation were confined to homogeneous fluids: but I did not so clearly know that the reasoning was equally restricted. I have since examined the reasoning with some attention; and I declare, that I cannot discover any part of it which is not as applicable to heterogeneous fluids as to homogeneous fluids. Judging from my own feelings, I think that the scientific world would be much obliged to Mr. Ivory, if he would point out the parts of his reasoning which are not applicable to heterogeneous fluids.

Mr. Ivory "consoles himself because he knows with the certainty of demonstration, that my problem is not solved, and cannot possibly be solved except by his theory." I console myself by thinking that Mr. Ivory has not reasoned with his usual accuracy upon a point which is somewhat abstruse, and by believing that my problem is solved (as far as such a problem can be solved) without the assistance of Mr. Ivory's equation.

I had intended to confine my remarks to the offensive note in which Mr. Ivory has treated me so unhandsomely. But as Mr. Ivory has in the preceding page mentioned another point on which we are at variance, I will endeavour to lay before your readers a more complete statement of the argument than he has given. I think it proper to say, that I have no reason whatever to complain of the terms in which he has there mentioned my name.

The first part of my paper (as Mr. Ivory has correctly stated) is employed in attempting to prove that Laplace's fundamental equation (*Méc. Cél.* liv. iii. No. 10) is exactly demonstrated,

monstrated, and for all kinds of spheroids differing little from a sphere. The only limitation of its generality is, that the sine or tangent of the angle made by the spherical and spheroidal surfaces at their intersection, must be expressed by a finite multiple of α; which condition is satisfied when y is expressed by any function, rational or irrational, that never makes $\sqrt{1-\mu^2}\,.\,\frac{dy}{d\mu}$ or $\frac{1}{\sqrt{1-\mu^2}}\,.\,\frac{dy}{d\omega}$ infinite. I have only to add, that this part of the paper is little more than a filling-up of the sketch given by Laplace in one of the last books of the *Mécanique Céleste.*

I cannot at present enter on the discussion of a very nice and abstruse point: I shall merely remark, that the difficulties which Mr. Ivory has found (see his paper, Phil. Trans. 1812, p. 16), appear to arise from the separation of $y' - y$ into two parts. For the rest I must beg leave to refer the reader to my paper in the Cambridge Transactions, vol. ii. "Now," says Mr. Ivory, "admitting that the equation in question is accurately and numerically proved, it seems impossible to deny that the series of terms deduced from it is numerically equal to the distance between the surfaces of the sphere and spheroid." With this I perfectly agree: but Mr. Ivory afterwards says, "Mr. Professor Airy, by supporting the fundamental equation without restricting it, and at the same time denying the unavoidable consequence, has only introduced new inconsistencies," &c. I can only infer from this that Mr. Ivory has not read the whole of my paper. However little the trouble of reading it might be repaid, it is not right to make such remarks on the connection of the first and the last parts, without examining or alluding to the subject which occupies the body of the paper. In the beginning I have endeavoured to show that the equation $-a\,\frac{dV}{dr} = \frac{2\pi a^2}{3} + \frac{V}{2}$ is generally true. From this the equation $4\alpha\pi a^2 y = \frac{U^{(0)}}{a} + \frac{3.U^{(1)}}{a^2} + \frac{5.U^{(2)}}{a^3} +$ &c. is derived by an unobjectionable process. But this equation as it stands is useless, unless we can resolve $4\alpha\pi a^2 y$ into a series of terms, distinguished by the same peculiarities which separate those on the other side of the equation. If it is not possible to resolve $4\alpha\pi a^2 y$ into more than one such series, the corresponding terms must be equal: if it is possible to do it in more than one way, nothing can be inferred from the equation, but the equality of the whole quantity on one side to the whole quantity on the other side. It is therefore necessary to prove that this resolution can be effected

fected in only one way. Now the most important part of my paper is occupied in endeavouring to show that the proof offered by Laplace is insufficient, and in giving a demonstration not liable to the same objections. Laplace's proof professes to be general; mine applies only to the cases in which y can be expressed (at any rate approximately) by a rational function of the coordinates. Where then is the unavoidable consequence of which Mr. Ivory speaks? I have endeavoured to show that the fundamental equation of Laplace is general, but that its application to the theory of the attractions of solids, is restricted by the limited nature of the proof of one of the subsequent steps. In this I can discover no inconsistency, nor do I perceive that I have embroiled the subject with new difficulties. I have only done with regard to one point, what Mr. Ivory has done respecting another: I have endeavoured to show that a demonstration professing to be general, is unsatisfactory, and have substituted one which appears, though more restricted, to be better founded.

I am sorry that I should have come in contact with Mr. Ivory, for the first time, on an occasion so disagreeable. I am not desirous of appearing in a public controversy of this nature; and under any common censure I should have remained quiet. But the manner in which I have been mentioned is so gross, and the name of the person who has mentioned me stands so high, that I have no other resource than to lay my defence before all who have read the accusation. I am aware, that the Editors of a Philosophical Journal can take little pleasure in inserting the squabbles of quarrelsome writers; and therefore, whatever further provocation may be offered, I shall not trouble you again with my complaints.

I am, Gentlemen, yours, &c.

Trinity College, Cambridge, May 9, 1827. G. B. Airy.

LXXXVII. *On a new Mineral Substance, proposed to be called Murchisonite.* By A. Levy, *Esq. M.A. F.G.S.*

In looking over some specimens of the conglomerate of the new red sandstone, which Mr. Murchison had brought from the neighbourhood of Dawlish, and which he was so good as to show me, I observed, in many of them, a felspar-like laminated substance, with a peculiar nacreous cleavage, which induced me to believe, it might differ from common felspar. Upon further examination I found that it had cleavages

ages in three different directions, two of which are at right angles to each other, like the two principal cleavages of common felspar, are obtained with the same facility, and present the same characters. The third has a nacreous appearance, is obtained as easily as the other two, and is found by the reflective goniometer to be perpendicular to one of them, and to make with the other an angle of 106° 50′. So that the solid obtained by cleavage is a tetrahedral prism, such as fig. 1, the incidences of the planes of which are as follow:

$$P, g^1 = 90^\circ. \qquad P, h^1 = 106^\circ\ 50'. \qquad g^1, h^1 \doteq 90^\circ.$$

This substance in the specimens from Dawlish, is white with a slight tinge of red, and is opake; it is accompanied by quartz, a little mica, and very minute crystals of black tourmaline disseminated throughout the mass. The whole forms rather a compact rock; but in some specimens the substance is partly or entirely disintegrated, almost pulverulent, and of a pure white colour.

Mr. Brayley jun. having kindly given me for examination several specimens he had himself collected, of the conglomerate of Heavitree, near Exeter, I found disseminated among the minerals and rocks which compose it, a great many crystals of this substance, always rounded on the edges, either slightly adhering to the red marl, or strongly attached to the more solid parts of the conglomerate. The form of these crystals is generally that represented by fig. 1, parallel to the planes of which they readily cleave.

Another form offered by these crystals is that represented by fig. 2: the plane *a* is always narrow, dull, irregular and curved, and its incidence upon P, measured by the common goniometer, is about 120°. Most frequently, however, these crystals are macled. Suppose two crystals of the form fig. 2. first placed in a parallel position, and in contact by their planes g^1 or penetrating each other; if then one of the crystals be supposed to turn round an axis perpendicular to the plane g^1 till its face P makes an angle of 128° 10′ with the face P of the other, they will be in the relative position of the two individuals which form the macles of this substance. The two nacreous planes are then inclined to each other at an angle of 161° 10′, and the plane *a* of one crystal is nearly on the same level as the plane P of the other: so that as these crystals are always so much rounded on the edges, and their planes so irregular, it is, in the greater number of cases, only by cleavage that it can be discovered that they are macles.

The nacreous cleavage of these crystals is not always so easily obtained, and frequently more interrupted than in the

 speci-

specimens from Dawlish; and instead of the silvery reflection of light of the latter, presents a gold-yellow reflection, generally not uniform but in spots. It somewhat resembles in this respect the variety of felspar called sun-stone, and when cut in a proper manner gives a similar play of light: but the red marl which is generally disseminated throughout the crystal, prevents the effect from being so great as it may reasonably be supposed it would have been had not that circumstance interfered. A further comparison between this substance and sun-stone would have been very interesting; but I could not procure the sight of a rough sun-stone, to examine whether it had any indication of cleavage; and the very high value set upon those which are cut, does not leave much hope of our being allowed to cleave them.

The natural plane itself parallel to the nacreous cleavage presents frequently a gold-yellow reflection. Simple and macled crystals are also found divided in two parts by a thin layer of red marl in a direction parallel to the nacreous cleavage, as if the crystal had been broken and cemented again. Small perihexahedral crystals of black mica are sometimes found in the interior of the crystals. Thin laminæ parallel to either of the two cleavages perpendicular to one another, are sometimes transparent. The hardness of the substance is rather less than that of felspar. Mr. Kent has been so good as to take with great care the specific gravity, and has found it to be 2·5091: I had found it somewhat lower, but I give in preference his result, as being determined with greater accuracy. Mr. R. Phillips has also had the kindness to analyse the substance, and has found the following result:

Silica	68·6
Alumina	16·6
Potash	14·8
	100·0

I have now to state the reasons which induce me to consider these crystals as belonging to a species distinct from felspar. The characters which are common to both are very apparent: they both possess cleavages in two directions perpendicular to each other; they have nearly the same hardness, nearly the same specific gravity; and the analysis, although indicating a greater proportion of silica and a smaller proportion of alumina than the adularia analysed by Vauquelin, and the common felspar analysed by Klaproth,—presents precisely the same result as the analysis of glassy-felspar by the latter. Finally, the macled crystals have a very remarkable resemblance to the macled crystals of felspar found in Auvergne

and

and in Bohemia, and represented in the translation of Mohs's "Mineralogy," by figures 80 and 81. For in both, the axis of revolution is perpendicular to one of the two cleavages at right angles to each other, and in felspar the second cleavage of one of the individual crystals forming the macle is inclined upon the second cleavage of the other, at an angle of 127° 3′, according to the dimensions I have assigned to the primitive form of felspar; whilst in the crystals under consideration, the same angle is about 128°.

The only difference which is now left to distinguish this mineral from felspar, whether in its laminar form, as in the specimen from Dawlish, or in crystals, as in those from Heavitree, is therefore the nacreous cleavage which it possesses, under both forms, and which cannot be obtained in felspar. But not only this cleavage does not exist in the varieties of felspar which have hitherto been examined, but it is not parallel either to any known modification of that substance, or to any unobserved modification, which might be derived by some simple law from the primitive form. To show the truth of what I have advanced, it is sufficient to observe, that in order to compare fig. 1, with a crystal of felspar,—for instance, with the figure given in Mr. W. Phillips's "Mineralogy," we must suppose that the plane P, fig. 1, corresponds to his plane P, and the plane g^1 to his plane M. Then we ought to find that the plane h^1 corresponds to either of his planes c^1, c^2, c^4, which are perpendicular to M, and inclined to P. But this is not the case: for these planes are respectively inclined upon P, at angles of 99° 15′, 129° 29′, and 146° 3′, whilst the inclination of the plane h^1 on P, fig. 1, is 106° 50′. Moreover, I find that a plane, the inclination of which upon P would be equal, or nearly equal, to 106° 50′, could only be derived by one of two laws, from the oblique rhombic prism, which is the primitive form of felspar, either by the law $o^{\frac{1}{10}}$ or $a^{\frac{2}{3}}$. Now neither of these modifications has ever been observed in felspar, and they are rather beyond the simplicity which might be reasonably expected in a modification parallel to which a cleavage is found to exist in some varieties. If now it is remembered that, as far as crystallographic observations go, it is found that, although some varieties of a species present occasionally cleavages which do not exist in all, in no occasion a cleavage has been obtained which did not correspond to some simple modification; even the false cleavage or faces of composition,—then the ground upon which I would propose to consider the substance I have just now described as distinct from felspar, will I hope become sufficiently obvious. The definition of the mineralogical

gical species which appears most consistent with the actual state of the science is, that a mineral species contains all the individuals composed of the same principles united in the same proportion, and when regularly crystallized, referable to the same primitive form. Now since cleavage has in every instance observed some very simple relation with the dimensions of the primitive, if we meet with a substance having a great resemblance to another, but having a cleavage that does not correspond to some simple modification of the primitive form of that other substance,—we must necessarily infer, that the first has a primitive form differing at least in its dimensions from the primitive form of the second, and consequently, according to our definition of the species, must constitute a new one. To the essential difference existing between the new substance and felspar, it may be added, that the first has no cleavage parallel to the lateral planes of the primitive form of the second, which most of the varieties of the latter present. But this difference would not be sufficient, since the facility of cleavage seems to vary with circumstances. Thus Mr. Faraday has discovered the means of obtaining crystals of sulphate of copper in which he may increase at will the facility of cleavage parallel to one of the primitive planes of that substance; so that he can even make it to crystallize in a mica-like state with a nacreous reflection of light on the face of the easiest cleavage.

I shall propose for the substance I have described the name of Murchisonite, in compliment to the gentleman who first directed my attention to it, and whose zeal for mineralogical science is so well known.

Fig. 1.

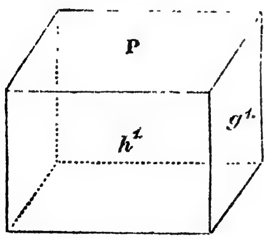
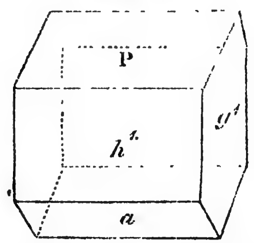


Fig. 2.



LXXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 5.—THE reading of Dr. Thomson's paper, On the compounds of chromium, was resumed and concluded. The principal object of this paper is to give an account of a singular

lar compound of chromic acid and chlorine, discovered some years ago by the author; but in the investigation to which it gave rise, the author was led to a more careful examination of the oxides of chromium than they had before undergone, and to a knowledge of their composition. The communication begins with a description of metallic chromium. That used by the author was reduced by Mr. Cooper. It was white, with a shade of yellow, very brittle, and sensibly attracted by the magnet even in fine powder; its specific gravity 5·993 at least. Nitric acid boiled on it has no effect; and aqua-regia scarcely any, unless the action be very long continued. When heated however with a mixture of potash and nitre, it is converted into chromic acid. The weight of its atom is 4.—The author next describes the green oxide of chromium, which consists of 1 atom metal, 4 and 1 oxygen 1 = 5. And after the description also of two compounds, the one of chromic acid and oxide of chromium, the other of sulphur and the same oxide, he proceeds to describe a combination of 4 atoms peroxide of iron and 1 green oxide of chromium. Phosphuret of chromium he states to contain $1\frac{1}{2}$ atom phosphorus and 1 atom metal. The brown oxide appears to be either a mixture, or a compound far from intimate, of 1 atom chromic acid and 6 atoms of oxide of chromium.

The next section of this paper is devoted to an account of the chloro-chromic acid, a remarkable compound produced by making sulphuric acid act on a mixture of 190 parts of bichromate of potash, and 225 parts of common salt. From this mixture, on applying heat, it separates in red fumes, and distils over in a liquid of a rich deep crimson colour, of a sweet, astringent, acid taste, and having a strong smell of chlorine. Specific gravity nearly twice that of water, with which it does not mix, but which decomposes it, evolving chlorine and producing heat. This liquid, when dropped into oil of turpentine or alcohol, or poured on sulphur, sets them on fire; but (which is remarkable) it not only does *not* fire phosphorus, but even extinguishes it when already inflamed. On other combustibles and metallic bodies it acts with great energy, but without producing ignition: in ammoniacal gas, however, it burns readily. When heated, *per se*, the chlorine escapes, and a substance resembling green oxide remains. Dr. Thomson analysed it by solution in water, saturation with carbonate of soda, and precipitation by solutions of baryta and silver; stating its composition to be an atom of chlorine and an atom of chromic acid.

An account of the salts of chromium occupies the succeeding portion of the communication. They are formed by the union of the green oxide with acids, and are all uncrystallizable and of very intense colours. They are not precipitated by sulphuretted hydrogen; gallic acid precipitates them green; prussiate of potash only changes their colour to brown, and throws down no precipitate; ammonia and potash throw down green oxide, which redissolves in excess of the latter.—The following salts are next described in detail: the muriate, nitrate, sulphate, dicarbonate, biphosphate, chromate, oxalate, tartrate, and potash-tartrate of chromium.—Dr. T. then gives an account of certain chromates not before

fore described; viz. the perchromate of iron, dichromate of lead and silver, double chromate of potash and soda, and double chromate of potash and magnesia. The author concludes this paper with an account of his analysis of the mineral known in cabinets as chromite of iron, which, when examined in a state of purity, he found to consist of 2 atoms of green oxide of chrome, 1 of peroxide of iron, and 1 of alumina, together with an admixture of a white matter, apparently a metallic salt, of unknown acid and base, but too minute in quantity for thorough examination.

The Society then adjourned to

April 26, when Dr. J. Blackman was admitted a Fellow, and H. R. H. the Duke of Clarence *elected* a Fellow, of the Society.

A paper was read, entitled, "On the derangement of certain transit instruments by the effects of temperature; by Robert Woodhouse, Esq. F.R.S. Plumian Professor of Astronomy in the University of Cambridge."

In the Philosophical Transactions for 1825, the author alluded to the derangement of the Cambridge Transit Instrument, arising from unequal expansion of its braces, establishing as he conceived the fact and cause of such derangement; and in a subsequent paper instanced its effects in one case as altering by no less than 20ˢ the time of the passage of the pole-star over the wires. The removal of the braces was in consequence resolved on; but from one cause or other, delayed; though the Author considers good to have arisen from this procrastination, as enabling him to make further experiments, which he was led to do, in consequence of Mr. South's observations, which led to conclusions opposite to those deduced by himself. To satisfy his own mind, therefore, he instituted the series of experiments described in this paper, from which he concludes that the partial heating of the diagonal braces, or of any one of them, deranges the Cambridge Transit Instrument according to the reasoning in his former paper; and that this cause may, in certain instruments, and under certain circumstances of temperature, produce balancing-effects, thus giving an appearance of inflexibility, which under other circumstances would not subsist.

LINNÆAN SOCIETY.

May 1.—A large collection of New Holland Birds and Quadrupeds, presented by Sir John Jamieson, were upon the table.

The Society proceeded to fill up the vacancies in the list of Foreign members, when the following gentlemen were elected:—Henry Ducrotay de Blainville; Charles-Lucien Bonaparte, Prince of Musignano; Leopold von Buch; Viscount Henry de Cassini; Henry Fred. Link, M.D.; C. F. P. von Martius, M.D.; C. G. Nees von Esenbeck, M.D.; Ch. Asmund Rudolphi, M.D.; Auguste de Saint-Hilaire; Frederick Teidemann, M.D.

A description was read of a new Genus belonging to the natural family of plants called *Nymphæaceæ;* by Nathaniel Wallich, M.D. F.L.S. superintendant of the Botanic Garden, Calcutta. The following is the character of the genus:

Hydrostemma: (POLYANDRIA POLYGYNIA.)—Sepala 5 distincta, infra

infra ovarium thalamo inserta. Torus basi in ovarium globosum ampliatus; inde tubulosus, corollaceus, staminiferus, fauce 8—10-lobo, lobis inæqualiter bi- vel tri-serialibus, conniventibus. Stamina plurima nutantia, tubo tori intus adfixa eodemque inclusa; superiora sterilia ramosa. Antheræ nudæ. Styli plures, fundo tubi radiatim inserti, supra foveam verticis ovarii conniventes, basi connati. Bacca carnosa, globosa, calyce suffulta corollâque coronata persistentibus, multilocularis, polysperma. Semina globosa, setis carnosis obtecta, albuminosa, inversa.

May 24.—The Anniversary was held on this day, as directed in the Charter, at the Society's House, A. B. Lambert, Esq. V. P. in the chair; when the following were chosen as Officers and Council for the year:—

President: Sir James Edward Smith, M.D. F.R.S., &c. — *Vice-Presidents:* Samuel Lord Bishop of Carlisle, LL.D. V.P.R.S. F.A.S.; A. B. Lambert, Esq. F.R.S. A.S. & H.S.; W. G. Maton, M.D. F.R.S. and A.S.; and Edward Lord Stanley, M.P. F.H.S.—*Treasurer:* Edward Forster, Esq. F.R.S. & H.S.—*Secretary:* James E. Bicheno, Esq.—*Under Secretary:* Richard Taylor, F.S.A. Mem. Astr. and Asiat. S. Also to fill the vacancies in the *Council:* Arthur Aikin, Esq. V.P.G.S.; John Barrow, Esq. F.R.S.; Francis Boott, M.D.; Mr. George Loddiges, F.H.S.; Richard Waring, M.D.—Many members of the Society afterwards dined together at Freemason's Tavern, Lord Stanley in the chair.

ASTRONOMICAL SOCIETY.

An Address delivered by J. F. W. Herschel, *Esq. President of the Astronomical Society of London, on the Occasion of the Distribution of the Honorary Medals of that Society, on April* 11, 1827, *to* Francis Baily, *Esq., Lieutenant* W. S. Stratford, *R.N., and Colonel* Mark Beaufoy.

Gentlemen,

The ordinary business of the evening being now terminated, it remains to fulfil the object for which we are especially convened this night, which is one of no less interest than the distribution of the Honorary Medals awarded by your Council, in pursuance of the principle of encouraging works of great labour, high practical utility, and steady perseverance in astronomical observation, and in redemption of the pledges held out in the Address circulated at the origin of this Society, explanatory of its objects.

On former similar occasions when we have been called on to witness the execution of this important duty, it has frequently been our good fortune to acknowledge and applaud the claims of foreign merit, and to prove by our awards, that no mean jealousies, or narrow and mistaken views of national honour, are capable of blinding our judgement or biassing our decision; but that he who, whatever be the spot of earth he inhabits, most promotes the cause of Astronomical science, is most our brother and our countryman. Yet, I am sure it will be gratifying to you to know that on this occasion, ample scope

scope has been found for selection in the merits of our own compatriots, and in the home list of our members. It is not that great and important Astronomical works have not emanated from our continental neighbours: on the contrary, the spirit of research and discovery appears to have prevailed with extraordinary activity; and the last year has even witnessed the addition to our system of another of those singular bodies, the discovery of which has conferred so much lustre on the names of Halley and Encke. No less than three independent claimants to the almost simultaneous disclosure of this interesting fact may be enumerated; and this circumstance, while it marks the spirit of the age more forcibly perhaps than any trait which could be produced, must obviously render it impossible for this Society to interfere or decide on the priority and rank of the competitors. But though unmarked by any tangible memorial of our approbation, the names of Biela, Clausen, and Gambart will not the less be cherished among us, and enrolled by posterity in the choicest and most permanent annals of Astronomical celebrity.

It is however for labours of a very different kind that our medals are this day to be conferred: labours, if less brilliant, yet more vital; if less associated with lofty speculations on the nature of the universe, yet more intimately linked with the practical uses of this world. The first award of your Council is that of a gold and silver medal respectively to your late excellent President Mr. Baily, and your indefatigable Secretary Mr. Stratford, for their joint labours in the construction of the Catalogue of 2881 principal fixed stars, which forms the Appendix to the second volume of the Memoirs of this Society.

A catalogue of stars may be considered in two very distinct lights, either as a mere list of objects placed on record, to fix on them the attention of astronomers, and to afford them matter for observation, or as a collection of well-determined zero points, offering ready means of comparing their observations with those of others, and of detecting and allowing for instrumental errors. In this light only I shall now consider it as chiefly of importance to the practical astronomer. It is for his uses that an amount of pains, labour, and expense, both national and individual, has been bestowed on the perfection of such catalogues, which on a superficial view must appear in the last degree lavish, but which yet has been no more than the necessity of the case demands. If we ask to what end magnificent establishments are maintained by states and sovereigns, furnished with master-pieces of art, and placed under the direction of men of first-rate talent, and high-minded enthusiasm, sought out for those qualities among the foremost in the ranks of science:—if we demand *cui bono?* for what good a Bradley has toiled, or a Maskelyne or a Piazzi worn out his venerable age in watching? the answer is,—not to settle mere speculative points in the doctrine of the universe; not to cater for the pride of man, by refined inquiries into the remoter mysteries of nature,—to trace the path of our system through infinite space, or its history through past and future eternities. These indeed are noble ends, and which I am far from any thought of depreciating;

ciating; the mind swells in their contemplation, and attains in their pursuit, an expansion and a hardihood which fit it for the boldest enterprize.—But the direct practical utility of such labours is fully worthy of their speculative grandeur. The stars are the land-marks of the universe; and amidst the endless and complicated fluctuations of our system, seem placed by its Creator as guides and records, not merely to elevate our minds by the contemplation of what is vast, but to teach us to direct our actions by reference to what is immutable in his works. It is indeed hardly possible to over-appreciate their value in this point of view. Every well-determined star, from the moment its place is registered, becomes to the astronomer, the geographer, the navigator, the surveyor,—a point of departure which can never deceive or fail him,—the same for ever and in all places, of a delicacy so extreme as to be a test for every instrument yet invented by man, yet equally adapted for the most ordinary purposes; as available for regulating a town clock, as for conducting a navy to the Indies; as effective for mapping down the intricacies of a petty barony, as for adjusting the boundaries of transatlantic empires. When once its place has been thoroughly ascertained and carefully recorded, the brazen circle with which that useful work was done may moulder, the marble pillar totter on its base, and the astronomer himself survive only in the gratitude of posterity: but the record remains, and transfuses all its own exactness into every determination which takes it for a groundwork, giving to inferior instruments, nay even to temporary contrivances, and to the observations of a few weeks or days, all the precision attained originally at the cost of so much time, labour and expense.

To avail ourselves of these records, however, we must first have the means of disentangling the observed places of the stars at any moment, from the regularly progressive effect of precession, and from a variety of minuter periodical inequalities arising from the nutation of the earth's axis, and from the aberration of light, of which the genius of theoretical, no less than the industry of practical, astronomers has at length succeeded in developing the laws, and fixing the amount, so as to leave little probability of any material change being induced by future researches.

The calculations, however, required for this purpose, if instituted for each particular star at the time it is wanted, are so numerous and troublesome as to become a very serious evil; the effects of which have been severely felt in Astronomy in the discouragement it has offered to the reduction of observations, owing to which the labour of many an industrious observer's life has been in great measure thrown away. Indeed, a lamentable picture might be drawn of the waste of valuable labour traceable to this cause. The want of tables therefore to facilitate the reduction of particular stars was early felt. I shall not, however, enter into any historical detail of the attempts hitherto made from time to time to supply this desideratum. A well drawn up and concise account of them is given in Mr. Baily's Preface to the Catalogue, which renders superfluous all I could say on the subject. Indeed, useful as they have been, and con-

considerable as has been the pains bestowed on them, they are all so far surpassed by this work of Mr. Baily, that it ought rather to be considered as belonging to a new class, than to be compared in any way with preceding ones, which must eventually all be superseded by it*.

It is time now to speak more particularly of the Catalogue itself. Its whole plan and arrangement, the selection of the stars, the preparation and revision of the formulæ, the choice of the coefficients, and the discussion of the terms to be retained or rejected, we owe to Mr. Baily, who has stated every particular relating to it in a most elaborate Preface, which may indeed be regarded as a compendium of all that is known on the subject of the corrections, and is remarkable at once for its precision and perspicuity. A great portion of the computation has been gratuitously performed by Mr. Stratford, checked by a computer engaged for that purpose. From this very severe labour, however, he was unfortunately compelled to desist, I regret to say by ill health, and his place supplied by a professional computer: but the hardly less laborious task of comparing and checking the computations of his assistants, and, what is as important in all such cases as accuracy of computation, the careful superintendence of the press, and repeated revision of the whole work, has entirely devolved on him; and never, I must say, was task performed with more diligence and exactness.

The selection of the stars has been made from the Catalogues of Flamsteed, Bradley, Lacaille, Mayer, Piazzi and Zach, so as to include all stars down to the 5th magnitude, wheresoever situated in the heavens,—all of the 6th within 30° of the equator, and all the stars to the 7th magnitude inclusive, within 10° of the ecliptic. Almost all of them, however, are to be found in the Catalogues of Bradley or Piazzi, from which they have been reduced to 1830, (the epoch adopted) by formulæ given by Bessel. Their number is so considerable, that in whatever part of the heavens we may be observing, one or more are sure to be within a moderate distance; so that no one provided with this Catalogue can possibly be at a loss for a zero-point to check his observations, and ascertain the state of adjustment of his instrument. To its convenience and utility in this respect, I can speak from individual experience. It is indeed become my sheet anchor, and has infused into a series of observations wholly dependent on such aid, a degree of exactness which, without it, I should hardly have expected to attain.

The formulæ employed for calculating the corrections are almost entirely those of Bessel, who has laboured with such diligence and perseverance on this department of Astronomy, as to make the subject almost his own. In adopting them, however, Mr. Baily has taken nothing for granted, even from such high authority. He has gone over the whole subject anew; and the slight inaccuracies which

* From this sentence, however, I ought to except special tables for the daily reduction of a certain number of select stars, whose use is no way superseded by the general Catalogue, being destined for continual, as the latter is only for occasional, reference.

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he has detected and corrected in several of the results of this profound geometer, although almost insensible in a numerical point of view, are valuable, as proving at once the general accuracy of his investigations and the minuteness of the scrutiny they have undergone.

The most delicate part of the whole operation, however, was the choice of the several coefficients, which, if erroneously assumed, would render the whole subsequent work of no value. In making this assumption, Mr. Baily has exercised a degree of judgement which I feel convinced will unite the suffrages of astronomers. Taking a comprehensive view of the results afforded by all former investigations, he has uniformly adhered to the principle, to steer clear of extreme quantities, and to adopt only such as not only rest on the greatest number of the best observations, but agree in their values nearly with the average of all. Thus, in the case of the aberration, the value adopted is the mean of the almost miraculously coincident results of Brinkley and Struve, and agrees within two-hundredths of a second with that of the extreme values assigned by Bradley and Bessel. I have much satisfaction in being enabled to state, that this value has been recently confirmed within a very minute fraction of a second, by the praiseworthy zeal and industry of Mr. Richardson of the Royal Observatory, who has compared for this purpose upwards of 2000 observations, made with the two mural circles of Jones and Troughton; so that this datum may be regarded as one of the best established in Astronomy. In the same cautious manner has Mr. Baily proceeded with the other coefficients. That of precession he has taken entirely from Bessel's elaborate investigations compared with those of Laplace, in which the only remaining source of uncertainty, is that arising from our ignorance of the mass of Venus; the influence of which cannot possibly produce an error, however, of a tenth of a second in the precession. The nutation he has taken, as it results from Dr. Brinkley's observations, which (like his aberration) justify this partiality by holding almost exactly an average value among all the different results of Bradley, Mayer, Maskelyne, Laplace, and Lindenau, and can hardly be considered as more than a tenth of a second in error.

This judicious choice will secure the present tables from a possibility of ever sharing the fate of preceding labours of this sort. They can never be superseded by others of greater accuracy, nor fall into disuse or grow obsolete till the apparent places of the stars shall have become so much altered by the effect of precession as to render the computations inexact, for which a very long series of years will be required.

But the distinguishing characteristic of this work, is the adoption throughout of Professor Bessel's capital improvement in the system of applying the corrections, by arranging the formulæ in such a manner that all that is peculiar to each star, and permanent in magnitude, shall stand distinctly separated from all that is ephemeral, or varying from day to day; and that in such a manner that a short ephemeral table, capable of being compressed into a single page, shall serve, not only for these stars, but for every star in the heavens.

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The convenience of this method, the brevity it introduces into the computations, the distinctness it gives to all the process of reduction, requiring neither thought nor memory on the computer's part, give it an incalculable advantage over every other. To reduce any observation, no other book need be opened. The work occupies four lines, and is done in half that number of minutes. If we compare this with the tedious and puzzling operation required by former processes, we shall fully agree with Mr. Baily that "those only who are versed in such calculations can appreciate the labour, the risk of error and the loss of time incurred in their several operations;" all which are saved by the present arrangement.

These considerations will amply justify the award of your council in your eyes and those of the world. They will justify a great deal more. At no time was the necessity of pressing on the attention of astronomers the utility, I may say, the duty of uniformity in their systems of reduction more urgent than at present*, when hardly a nation in Europe is unprovided with a good observatory, and when rival astronomers in all quarters of the globe are contending for the palm of accuracy and diligence. So long as they persist in continuing to reduce their observations by different systems, their merits can never be fairly compared. Each may boast the perfection of his instruments, and vaunt himself in the security of his preeminence. Each may promulgate his standard Catalogue, which will be adhered to in his own nation, and rejected by all others; thus dividing astronomers into sects and parties,—a state of things which ought surely not to continue. The only remedy is to agree to speak one language, to adopt one system. It matters little in the present advanced state of science, whether that system be still open to infinitesimal corrections. Let astronomers only consent to use it as, like all human works, confessedly imperfect, and in process of time to be corrected: but not at the caprice of each individual who may think one coefficient a tenth of a second too small, or another as much too great; but after full consideration, when the necessity and amount of correction shall have become certainly known and generally agreed on.

Meanwhile, a fair opportunity is offered to rival astronomers throughout the world, to try their strength, in an arena of ample extent, and where every part of the honourable contest will be brought distinctly into sight. In giving this Catalogue to the world, we invite their examination to its errors, (for such it must contain,) and call on them to lend their aid to its perfection, by determining, with all the exactness their resources afford, the mean places of the stars it comprises. For this, its arrangement affords every facility, and those who observe, have no excuse for neglecting to reduce. Let us hope then, that instead of lavishing their strength in fruitless attempts to give superhuman precision to fifty or a hundred select objects, the formation of a standard Catalogue of nearly 3000 will be deemed of sufficient importance to fix the attention of astronomers;

* This applies with equal or greater force to the correction for refraction; a common table for which ought to be agreed on and adhered to by all.

mers; and that not only those to whom the direction of great national observatories is confided, but even private individuals, if such there be, who feel themselves in possession of the means required, may take a share in this glorious, but at the same time arduous undertaking.

(*The President then, delivering the Gold Medal to Mr. Baily, addressed him as follows:—*)

Mr. Baily,

Accept this Medal, which the Astronomical Society bestows on you, by an award which every astronomer in Europe will confirm. The work you have accomplished will identify you with the future progress of that Science, into almost every department of which it is calculated to infuse new life; since every practical astronomer has in it to thank you for an accession of power. It is needless for me to accompany this testimony of the sense the Society entertains of your distinguished merits, with the expression of a hope that your exertions in the cause of Astronomy will continue. You could not struggle against nature so far as to desist from pursuits which, demanding of ordinary men a total devotion of their time, and concentration of their whole intellectual powers, have been to you a relaxation from the most active business. Possessing thus within yourself a source of pure and exalted enjoyment, enhanced by the consciousness of public utility, and a certainty of the approval and admiration of those whom you esteem, we can only add our wishes that length of years, and continuance of health, may render your distinguished talents, and rare zeal for the promotion of your favourite science, as useful to Astronomy as it is honourable to yourself.

(*The President next presented the Silver Medal to Mr. Stratford, addressing him at the same time in these words:—*)

Mr. Stratford,

The Medal which, in the name of the Astronomical Society, I now deliver to you, though "less fine in carat" will, I trust, be to you "more precious" than gold, as proving how highly we appreciate your devoted and persevering attention to the work you have so happily brought to a conclusion. Those only who have actually entered into the details of a work of this nature can possibly understand the overwhelming, and soul-sickening labour of such a task; —but the pile of volumes now lying on the table, a great portion of which you have yourself penned, and the whole of which you must in the course of your undertaking have repeatedly read over, figure by figure, will serve to give some idea of it. In executing this arduous duty, you have had no other inducement than your zeal for the progress of science, and that devotion to the interest of this Society which is so conspicuous in every part of your conduct, and which would not suffer you to tolerate the idea of any incorrectness, anything unworthy the importance of the subject emanating from it. The habits of correctness in numerical computation, and systematic fidelity of detail indispensable for such a work, you possess, though in perfection, yet in common with many: but the

the enthusiasm in the cause of abstract science, which could carry you successfully through the task thus voluntarily imposed on yourself, you share with few. You have however the satisfaction of knowing that so much labour has not been bestowed in vain; for, if there be any thing on which we can calculate with certainty, it is that the work you have been mainly instrumental in completing, must exercise a powerful influence on the future destinies of Astronomy.

(The President then resumed his Address to the Members in general, as follows:—)

GENTLEMEN,

We have still another, and a very interesting part of the business of this meeting to perform, in the delivery, to Colonel Beaufoy, of a Medal for his valuable series of observations of eclipses of Jupiter's satellites, communicated to this Society, and in part already printed in the first part of the second volume of our Memoirs; in part recently read at a late meeting, and completed up to the present time, by the paper you have heard read to-night.

The subject of the eclipses of Jupiter's satellites, is one of singular interest in the history of Astronomy. The discovery of these bodies was one of the first brilliant results of the invention of the telescope; one of the first great facts which opened the eyes of mankind to the system of the universe—which taught them the comparative insignificance of their own planet, and the superior vastness and nicer mechanism of those other bodies, which had before been distinguished from the stars only by their motion, and wherein none but the boldest thinkers had ventured to suspect a community of nature with our own globe. This discovery gave the holding turn to the opinions of mankind respecting the Copernican system: the analogy presented by these little bodies (little however only, in comparison with the great central body about which they revolve) performing their beautiful revolutions in perfect harmony and order about it, being too strong to be resisted. As if to confirm this analogy beyond dispute, Kepler lived just long enough to witness the discovery, and to demonstrate* the extension of the same general law to their periods which he had found to obtain among those of the primary planets about the sun. The conclusion was irresistible; and the full establishment of the Copernican System must date from the discovery of the satellites of Jupiter.

This elegant system was watched with all the curiosity and interest the subject naturally inspired; and the eclipses of the satellites speedily attracted attention, and the more when it was discerned, as it immediately was, by Galileo himself, that they afforded a ready method of determining the difference of longitudes of distant places on the earth's surface by observations of the instants of their disappearances and reappearances simultaneously made. Thus the first astronomical solution of the great problem of the longitude,—

* According to Delambre this extension of Kepler's law is due to Vendelinus.

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the first mighty step which pointed out a connection between speculative Astronomy and practical utility, and which, replacing the fast dissipating dreams of astrology by nobler visions, showed how the stars might really and without fiction be called arbiters of the destinies of empires,—we owe to the satellites of Jupiter; to those atoms, imperceptible to the naked eye, and floating like motes in the beam of their primary—itself an atom to our sight—noticed only by the careless vulgar as a large star, and by the philosophers of former ages as something moving among the stars—they knew not what—nor why; perhaps only to perplex the wise with fruitless conjectures, and harass the weak with fears as idle as *their* theories.

No wonder now that the eclipses of the satellites were watched with anxious, earnest interest; they were soon to afford matter for yet greater wonder and deeper contemplation. Roemer's discovery of the velocity of light from the retardation of their eclipses about the end of the 17th century, was the next in order, and the sublimest truth they were destined to be the means of unfolding; a truth so amazing, so overwhelming to human faculties, that (not to mention the feebler names of Cassini, Maraldi and Fontenelle) even the comprehensive genius of a Hooke quailed before it, and refused to admit the existence of a motion so little short of infinite in a finite system like our own. The discovery of the aberration of light by Bradley, however, more than 40 years afterwards, confirmed it in its full extent; and no truth in the circle of physical science is either more astonishing, or better established than this.

We are not yet come to the end of the long catalogue of useful and admirable results afforded to science and to mankind by the discovery of these bodies. We have hitherto regarded only obvious results; broad and evident conclusions from apparent facts. Let us now trace them in the quiet succession of their convolutions, in the unfolding of their periodical inequalities, in the slowly accumulating amount of their mutual action, in the influence of the oblate figure of their primary on their orbits; in short, through all the mazy intricacies of their perturbations. The lessons they have thus whispered to the intellect of man, over the midnight lamp, have not been less instructive, less fraught with wonder and utility, than those which they have blazoned to his senses. It is to that powerful and gifted genius, now so recently gathered in an illustrious grave; on whose ashes the tears of mourning science are yet warm,—to him, whose revered name so freshly sanctified by death, I am unwilling to pronounce, that we owe the complete development of their theory. His penetrating mind saw all the advantages likely to accrue to the general theory of the planetary perturbations from the study of this miniature system, where years are represented by days, and ages by years, and where inequalities, which in the planetary theory have a character approaching to secular, can be traced in their increase and on their wane. Aided, therefore, by his powerful analysis, he succeeded in applying the law of gravitation to the minute investigation of all their inequalities; and the result has been not merely another triumph of the Newtonian theory in the complete explanation of all their complicated irregularities, but

but the formation of tables even more perfect than observation itself*: and in addition, a mass of most valuable and instructive information on the general nature of planetary perturbations, amply repaying all the labour of the inquiry, and adding fresh lustre to the already imperishable glory of his name.

This slight sketch of the history of the satellites of Jupiter may serve to show how intimate is the connection of distant parts of science with each other, and that in it we are to regard nothing as trivial and nothing as great in itself, but in respect of the instruction we may draw from it;—to show, in fine, how deep are the foundations and how wide spread the ramifications of that tree of knowledge which, in the poet's words,

> . . . . quantum caput ardua ad astra
> Attollit—tantum radice in Tartara tendit.

which draws its increments from small beginnings and matters of speculative curiosity, and ends in becoming the ornament, the shelter, and the support of society.

It is by observations of the eclipses of the satellites alone that their theory can be compared with nature, their apparent distances from the planet being too small and its change too slow to admit of micrometrical measurements precise enough for the purpose, though perhaps the modern improvements both in the telescope and micrometer may authorize a hope that this may not long be an insuperable difficulty. Accordingly, from the time of Roemer downwards, a series of eminent astronomers have occupied themselves with observations of these phænomena, and it is on no less than two thousand of such observations that Delambre, improving on the tables of Wargentin by the aid of the profound theory just alluded to, succeeded in calculating the first series of tables laying claim to precision.

The longitude is so much better ascertained now by lunar distances and occultations, that these observations are less resorted to than heretofore for that purpose. Nevertheless they are occasionally used, especially those of the first and second, whose eclipses not only happen much more frequently, but are much more definite, than those of the exterior ones. Indeed, the observations of the latter have been declared by high authority, utterly useless. It is not always good, however, to trust to authority; and Mr. South by a comparison of his own with Colonel Beaufoy's observations, has arrived at a very different conclusion, at least for the cases when both the beginning and end of the eclipse can be seen. Still, however, it is highly desirable that they should continue to be assiduously observed, not merely to furnish corresponding observations, but to afford the means of further perfecting the tables, so as ultimately to enable us to dispense with corresponding observations altogether.

Colonel Beaufoy has for many years past been a most careful and assiduous observer of these eclipses and indeed of all occasional phænomena; such as occultations, eclipses both Solar and Lunar,

* Than any single observation.—*Delambre.*

and

and of late of that very useful and important class, the transits of Moon-culminating stars, of which one of his recent communications contains an extensive and highly interesting series. His observations of the immersions and emersions of the satellites communicated to this Society, amount to no less than 180, all (with the exception of two or three of the earlier ones) being made in the interval from 1818 to 1826 inclusive;—a fine series, indeed a surprising one, when the comparative rarity of the phænomena is considered, not more than about 40 visible at Greenwich occurring annually on an average, and when the great drawback on observations of this sort from unfavourable weather in this anti-astronomical climate is taken into the account. What chiefly adds to their value as a series, however, is the circumstance of their being all made by one observer, and with one telescope,—a fine five-feet achromatic of Dollond, and with the same magnifying power 86. In no class of Astronomical observations, is uniformity in this respect of such importance, since the variations in the times of appearance and disappearance, when observed at the same spot, simultaneously, by different observers with different telescopes, is found to amount not merely to a few seconds but to whole minutes.

It must be a matter of deep regret to us all, both for his own sake and for that of Astronomy, that so valuable and interesting a series of observations should sustain what I trust however will prove only a temporary interruption from the severe illness of Colonel Beaufoy, which alone prevents him from receiving in person the mark of our approbation adjudged him by your Council. At his request, therefore, I will hand it to our worthy Secretary.

(Here the President delivered the Medal to Mr. Stratford, as proxy for Colonel Beaufoy, at the same time thus addressing him:)—

Mr. Stratford,

When you shall transmit this Medal to Colonel Beaufoy, accompany it with the assurance of our warmest approbation of the useful and excellent example he has set, in thus steadily prosecuting from year to year, a train of observations so important in itself and requiring so much patient and persevering attention: an example we trust to see emulated by others, since it shows how much, how very much, may be done with moderate instrumental means, by regular, systematic, and well directed observation. He has succeeded in rendering his name conspicuous among astronomers, and his observatory a standard point of reference,—one of those zero points on earth which, like the standard stars in the heavens, will serve for the determination of innumerable others. Already we are furnished with a conspicuous instance of its use in this respect, in the determination of the Longitude of Madras by Mr. Goldingham, which has this night been read to the Society, in which that important element is derived from a very moderate number of corresponding observations made at the two stations, with considerable presumption of exactness. Nor can we suppose that this will prove a solitary instance. Assure Colonel Beaufoy how much we consider science

as

as practically benefited by his labours:—assure him too of our lively grief and sympathy for his present sufferings, and our earnest wishes and prayers that he may be speedily restored to the full enjoyment of health, to his friends, and to his favourite astronomy.

HORTICULTURAL SOCIETY.

April 17.—The following paper was read: Upon the use of an infusion of tobacco for washing fruit-trees infested with *Aphides;* by Sir George Stewart Mackenzie, Bart.—A statement was read of the medals which had been awarded by several provincial societies in conformity with the determination of the Horticultural Society of London, to give annually one silver medal to some one person within the district of each provincial Horticultural Society, who shall appear to be deserving of it. A great variety of flowers and fruits were exhibited, and several kinds of seeds and cuttings were given away.

May 1.—This was an anniversary meeting, at which the following officers were elected:

President: Thomas Andrew Knight, Esq.—*Vice-Presidents:* John Elliot, Esq.; Dr. Henderson; R. H. Jenkinson, Esq.; Sir Claude Scott, Bart.—*Secretary:* Joseph Sabine, Esq.—*Vice-Secretary:* Edward Barnard, Esq.—*Assistant Secretary:* Mr. John Lindley.

May 8.—The following papers were read:—Observations upon canker in fruit-trees; by Mr. Archibald Stewart.—Observations on the effect of frost upon various hardy trees and shrubs at Newark; by T. C. Huddlestone, Esq.—On the cultivation of figs in Denmark; by Mr. P. Lindegaard.—On the use of double windows in hot-houses; by Mr. Frederick Otto. A fine display of fruits and flowers ornamented the table, and the usual distribution of seeds took place.

ZOOLOGICAL SOCIETY.

The anniversary meeting of this Society took place on Saturday; the Marquis of Lansdowne (President) in the chair. The meeting was very numerously attended. Amongst other distinguished supporters of the establishment, we noticed Earls Spencer, Malmesbury, and Carnarvon, Lord Auckland; Marquis Carmarthen; Bishop of Bath and Wells; Sir E. Home, Sir R. Heron, Bt. M.P., Sir T. D. Acland, Bt., Sir John De Beauvoir; Mr. Baring Wall, M.P., &c. &c. The President having adverted with much feeling and effect to the vacancy occasioned by the lamented death of the late President, and his own accession to that office, reported to the meeting the progress of the Society during the past year; from which it appeared that the Museum had been enriched by numerous and valuable donations; amongst the most conspicuous of these was particularized a female ostrich from His Majesty. The magnificent collection of Sir T. S. Raffles, consisting of mammalia, birds, reptiles, insects, zoophytes, &c. has also been transferred to the Society. The President further informed the meeting that the works in the Regent's Park are rapidly advancing: the walks are laid out and partly executed; and some pheasantries and aviaries,

with

with sheds and inclosures for some of the rarer animals belonging to the Society, are in active progress. It is expected that the gardens will possess sufficient interest to authorize the opening of them during the ensuing autumn. The President then announced that the number of subscribers exceeds 500, and that the list is daily increasing. He also gave a highly favourable report of the funds of the Society; which after defraying all charges attending upon the various works in progress, leave a considerable and increasing balance in the bankers' hands.

ROYAL INSTITUTION OF GREAT BRITAIN.

April 27.—Dr. Granville gave an account of his investigations of the processes followed by the Egyptians in the embalming of their mummies. After recapitulating what he had done in the examination of a very fine mummy, an account of which is already before the public, and referring also to other modes in which human bodies had been preserved, he proceeded to show how far his own exertions in preparing mummies, according to that which he considered as the Egyptian method, had been attended with success; and he produced numerous well preserved specimens prepared by immersion in tanning liquors, in saline solutions, &c. An extraordinary and abundant collection of mummies and preserved specimens was upon the table.

In the Library was a piece of a ship's bottom pierced by a sword-fish, presented by General Hardwicke; a poisoned arrow from Celebes; specimens of the *Mantis*, and numerous literary curiosities.

May 4.—Mr. Faraday gave an account of the chemical action of chlorine and its compounds when used as disinfectants. This was a continuation of the subject of February 2, by Mr. Alcock. The chemical action of chlorine upon infectious or fetid vapours appears to vary according to circumstances, sometimes apparently abstracting hydrogen to form muriatic acid with it; at others producing triple compounds of chlorine, carbon, and hydrogen; and at others again, probably decomposing water and causing the nascent oxygen to act upon the fetid or injurious substance;—but in all cases chemically altering its nature, and rendering it innocuous or very nearly so. The compounds of chlorine with lime and with carbonate of soda were considered as acting precisely in the same manner as pure chlorine, but with moderated energy; and the experiments of Gaultier de Claubry quoted as decisive on this point. The nature of the compound of chlorine with lime was considered as well ascertained; but that of the compound with carbonate of soda stated as doubtful. It is evidently not a chloride of soda; and when made according to the proportions directed by M. Labarraque, not a particle of carbonic acid is evolved. It was stated that in some experiments made by Mr. Phillips, a portion of this compound being evaporated, crystallized in acicular forms, which if dissolved still possessed disinfecting and bleaching powers, especially when carbonic acid was passed through the solution; but which, if exposed to the air for a sufficient time, lost all bleaching power; and being then dissolved, neutralized, and examined by nitrate of silver,

gave so little chloride of silver, as to show that scarcely any portion (if any) of chloride of sodium had been formed, and that by such exposure nearly all the chlorine could be liberated from the carbonate of soda.

The mummy of an Ichneumon was also opened this evening upon the Lecture-table, by Dr. Granville; and numerous rare and curious books were on the Library tables.

May 11.—Mr. Brockedon has devised a process by which fine metallic wires can be drawn through gems which, much surpassing the steel plates in ordinary use, suffer no appretiable wear, and permit an immense length of wire to be drawn without any increase in diameter. Many curious observations arose during the necessary experiments relating to the ductility, tenacity and malleability of metals. Mr. Brockedon described these at the Lecture-table, and illustrated the points of interest by numerous experiments.

In the Library were Mr. Wheatstone's Kaleidophone, or Phonic Kaleidescope; Mr. Lydiatt's Smeerologometer, an instrument to measure the tenacity of fine wire: and literary novelties.

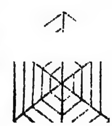
May 18.—Mr. Holdsworth gave a discourse on the forms of the hulls of vessels. The means of conveyance upon the waters was traced from the raft and balza upwards, to the most perfect specimens of naval structures; the various points of difference and coincidence being illustrated by numerous very fine models from the Navy Board and from private gentlemen, and also by drawings.

A series of Geological specimens, collected by Capt. Parry and his companions from Port Bowen, Prince Regent's Inlet, were laid on the Library tables, with many literary novelties and curiosities.

LXXXIX. *Intelligence and Miscellaneous Articles.*

CRYSTALLIZED CARBONATE OF POTASH.

M. FABRONI (Annals of Philosophy, N. S. vol. vii. p. 470) obtained this salt by evaporating a solution of it until its specific gravity was 1·6; it then deposited crystals in the form of long rhomboidal laminæ.



In order to procure crystallized carbonate of potash, I evaporated a solution of sp. gr. 1425 to about one half; on cooling, crystals were plentifully deposited, which having been examined by my brother W. Phillips, he states that the salt is so deliquescent that it is impossible to determine its form. It generally consists of a number of crystals, having a resemblance to the dog-tooth spar in form, arranged in the same direction, and limited externally by six sides; the underside of the crystal, represented in the accompanying figure, has a line proceeding to the centre from each of the six angles, and each of the six parts was striated in the same manner: viewing the whole as one crystal it would be said that the edge was replaced.

To

To determine the quantity of water of crystallization, I heated 200 grains of the salt to redness; they lost 42 grs.; consequently 158 of carbonate of potash are combined with 42 of water: therefore 70 = 1 atom give 18·6 of water, so little exceeding two atoms, that we may safely consider the crystallized carbonate of potash as composed of one atom of carbonate of potash = 70
two atoms of water . . . . = 18

Weight of the atom . . 88 R. P.

ACTION OF ÆTHERS ON VARIOUS BODIES.

M. Henry, senior, has made numerous experiments on the above-named subject, and has arrived at the following conclusions:—first, with respect to sulphuric æther. In this the easily oxidable metals and oxides, capable of combining with acetic acid, give rise to the formation of acetates, probably by decomposing, *not* the sulphuric æther, but the acetic æther which is always found in it. The author concludes also, that it is owing to the saturation of the acetic acid set free in consequence of this decomposition, that sulphuric æther during evaporation does not redden litmus paper, whilst it acts differently when exposed to a gentle heat; this small quantity of acetic æther with which it is mixed is decomposed by the action of the air, when it has not been combined with oxides.

Phosphorus and sulphur dissolve in sulphuric æther at common temperatures, especially the former, and in considerable quantity; protomuriate of iron is also soluble in it, and crystallizes from it in rhomboids of an emerald green colour. Nitric and acetic æthers are readily decomposed by keeping, by many substances without the assistance of heat, so as to occasion among other products the formation of their respective acids and acetates, and also alcohol which dissolves the salts formed: this is a fresh proof that their elements, though recently combined, may be very readily separated.

The results yielded by acetic æther have great analogy with those obtained with sulphuric æther, which is a reason for supposing that the opinion stated with respect to the acetification of the latter is very probable.

Muriatic æther dissolves phosphorus and sulphur to a certain extent.—*Journ. de Pharm.* March 1827, p. 130.

CHLORIDE OF BORON.

M. Dumas prepares this compound by passing dry chlorine gas over a mixture of charcoal and boracic acid, heated to incandescence in a porcelain tube. The tube was first heated to expel all moisture from the mixture, and the gas was then passed over it. When it had passed for about a quarter of an hour, an adopter and bent tube were attached, and the chloride was received over mercury. It is a gaseous body, and corresponds in composition with fluoboric acid; it is colourless, denser than air, fuming in contact with it, decomposable by water, and resists a high temperature. M. Despertz

spertz also lays claim to the discovery of this compound; he obtained it as above described, and also by passing chlorine over boruret of iron.—*Ann. de Chim.* xxx. 378—442.

NEUTRALIZING THE MAGNETISM OF WATCH-WORKS.

Mr. Abraham, of Sheffield, has contrived an extremely easy and effectual mode of divesting watch-works of their magnetism. The process consists in dipping the part to be divested of magnetism, as a balance-wheel, into fine steel filings, and then presenting a fine magnet to the part covered with them, at a distance of a quarter to one inch, according to the power to be neutralized. It will be directly observed whether the polarity of the magnet be of the same kind as that in the apparatus; if so, the filings will gradually fall from the part as the power becomes neutralized. When all the filings have fallen from the part submitted to experiment, dip it again into the filings, to prove whether it has acquired opposite polarity by remaining too long exposed to the magnet; if that be the case, present the contrary end of the magnet at a distance proportional to the power to be diffused. By this process, exposure to heat is rendered unnecessary.—*Trans. Society of Arts*, 44—59.

NEW ACHROMATIC TELESCOPE: BY M. CAUCHOIX.

M. Cauchoix, the optician, of Paris, has nearly completed an achromatic telescope, measuring about nineteen and a half feet in length, with an object-glass by the late M. Guinand, of 12¾ inches diameter. Some remarkable observations on Saturn's ring have already been made with this instrument, by MM. Arago and Mathieu, the results of which will shortly be published when fully verified.

CHLORIDE OF ARSENIC.

Put one part of arsenious acid and 10 parts of concentrated sulphuric acid into a tubulated retort, and raise the temperature to nearly 212° Fahr.; then throw fragments of fused common salt into the retort by the tubulure. By continuing the heat and successively adding common salt, protochloride of arsenic is obtained; it falls drop by drop from the beak of the retort, and may be collected in cooled vessels: little, if any, muriatic acid is disengaged, but towards the end of the operation a portion of hydrated chloride of arsenic is frequently produced, which collects in the vessels above the pure chloride. The two bodies do not mix; the hydrate is liquid, transparent and colourless, and more viscid than the dry chloride. The hydrate may be decomposed, and pure chloride obtained, by distilling the mixture from a sufficient quantity of sulphuric acid.—Dumas. *Ann. de Chim. et Phys.* xxxiii. 360.

NOTE RESPECTING MR. BABBAGE'S LOGARITHMS.

The logarithm of the number 24626, whose four last figures are 3939,

3939, is given among the errata printed at the end of the preface, to Mr. Babbage's Logarithms.

The errata so stated can scarcely be considered as errors, since each copy contains the proper corrections. The history of that particular mistake may be useful as pointing out the manner in which they are sometimes introduced. Its origin in all the modern tables arises from a misprint in Vlacq's folio edition of 1628, in which a nine is printed instead of an eight in the 7th place of figures. In the first three readings of the proofs of Mr. Babbage's tables, they were compared with tables corrected by his own copy of Vega, and this correction was included; and it was rightly printed 3939. During the next three readings by a different set of readers, copies of tables were accidentally employed, in which this had been neglected to be corrected; it was consequently altered to 3940. The plates were now stereotyped, and in the 7th and 8th reading it was again detected, and the source of its introduction traced. The only error at present known in these tables is the misprint in the logarithms of the number 13588, in which the fourth figure is a large unity instead of a small unity.

SILICA IN SPRINGS IS DISSOLVED BY MEANS OF CARBONIC ACID.

Dr. Karsten remarks, that, if so feeble an acid as the acetous, is capable of dissolving silica, it is not improbable that the carbonic acid may have the same property. This conjecture he has confirmed by experiment. The experiment may be made as follows. Decompose a portion of liquor silicum by means of a superabundance of any acid, the muriatic for example, and neutralize the clear fluid with carbonate of ammonia, at the lowest possible temperature. The carbonic acid evolved by this process combines with the water; and, if the neutral fluid is preserved in a well-closed glass vessel, it may be kept for many weeks, without exhibiting any precipitation of silica. But if it is exposed to the air, or, better, if the solution is heated in an open vessel, it is decomposed in proportion to the escape of the carbonic acid, and the siliceous earth is deposited on the walls of the vessel in a gelatinous state. This result shows, that the great quantity of silica met with in many mineral springs, particularly hot springs, is held in solution by carbonic acid. It is true that we cannot in this way explain how the siliceous earth was first dissolved,—for the generally received opinion, that the earth is simply washed out of the strata in the vicinity of the springs is, according to Karsten, untenable.—*Edin. Phil. Journ.*

NOTICE REGARDING THE COMMON STAR-FISH, ASTERIAS RUBENS.

On the 6th of March last year, M. Eudes Deslongchamps observed the beach at Colville to be covered with star-fish. When the waves retired, and there was still an inch or two of water upon the sand, he saw them rolling out in the form of balls, which, on examination, he found to consist of five or six individuals, closely united and

and clinging together by their rays. In the centre of each of these balls was a full-grown specimen of *Mactra stultorum*. The asteriæ were arranged along the edge of the valves, which were always separated to the distance of two or three lines; they were applied to them by their lower surface. On detaching them from the shell, it was remarked, that they had introduced between its valves, large round vesicles, with very thin walls, and filled with a transparent fluid. Each asterias presented five pendent vesicles, arranged symmetrically about the mouth. These vesicles were of unequal size: two of them were commonly larger, and about the size of a very large hazle-nut; the other three were not larger than a pea. They appear to be connected with the animal by a very short and narrow peduncle. At the other extremity was a round open hole, through which the fluid, contained in the vesicle, flowed gently, and drop by drop. The walls of these vesicles were very thin; the upper half, however, was thicker than the other, and longitudinally wrinkled. At the end of a few seconds, the vesicles, having contracted and discharged their contents, were scarcely larger than a grain of ordinary shot. When the sea had left the asteriæ some moments dry, they quitted the animal which they were in the act of sucking, and immediately after, the place of the vesicles could no longer be distinguished. The shells, that had been seized upon by these animals, were found in various states of destruction; some so far gone as to have only the adductor muscles remaining; but all of them had lost the faculty of closing their valves, and appeared to be dead. If testacea be the ordinary food of the asteriæ, an enormous quantity of them must be destroyed, if we may judge by the number of these animals. M. Deslongchamps inclines to the opinion that the asteriæ attack the mactræ while the latter are still alive, and that, probably, by means of some fluid, capable of producing torpor, they force them to open their shells, and thus allow the introduction of the singular bodies described, and which act as suckers. He is the more inclined to think so, as none of the mactræ, which he examined, had the least smell, or presented any other indication of having been dead for any time. It must, however, be remembered, that bivalve shells of this, or any other analogous species, tossed about by the waves, are no longer in their natural state, but have been raised from their native haunts under the sand, either by boisterous weather, or after intense frost, by even a scarcely more than ordinarily troubled state of the sea. Shells in this state are frequently observed on our shores. In some the animals are dead, in others so much weakened, as to be unable to close their shells, while others may, at least after gales, be for a time apparently as sound as ever. Now, it is more than probable, that the asteriæ could only attack those which were absolutely dead or dying, and from which the insertion of their suckers could experience no opposition; for it would be impossible for them to insinuate even a pretty solid substance, much less a mere vesicle, between the closed valves of a living shell; and, on the other hand, how should the asteriæ contrive to make the shell of a vigorous animal open, in order to let them throw in their imagined torporiferous fluid?—*Ibid.*

SUGAR OF MELONS.

M. Payen has lately extracted $1\frac{1}{2}$ of well crystallized sugar, and possessing all the properties of that of the sugar-cane, from 100 parts of the juice of the melon.—*Bull. Philomath.* 1826. p. 135.

FAILURE OF THE SUSPENSION BRIDGE AT PARIS.

The suspension bridge erected over the Seine at Paris, opposite the Hôtel des Invalids, by M. Navier, *Ingénieur des ponts et chaussées*, has entirely failed; the attachments of the chains having given way, on account of an error which it is conceived an engineer of inferior mathematical attainment to M. Navier would readily have avoided: and the circumstance is the more remarkable, as the construction of the suspension bridges in England (none of which has failed) has repeatedly been condemned by M. Navier.

LIVING CONDOR AT PARIS.

In the menagerie of the *Jardin des Plantes* is a living specimen of the condor, which has survived the past winter, and is now in a healthy condition.

SCIENTIFIC BOOKS.

Just Published.

Tracts on Hydraulics, edited by Thomas Tredgold, civil engineer; containing, I. Smeaton's experimental papers on the Power of Water and Wind to turn Mills, &c. &c.; II. Venturi's experiments on the Motion of Fluids; and III. Dr. Young's Summary of Practical Hydraulics, chiefly from the German of Eytelwein;—with notes by the editor, and illustrated by seven plates.

No. IX. commencing the third volume, of the Zoological Journal, containing a Memoir of the Life and Writings, and Contributions to Science, of the late Sir T. Stamford Raffles; with other original articles in every branch of Zoology, Reviews of Books, &c.

Preparing for Publication.

The Rev. J. A. Ross is preparing a Translation from the German of Hirsch's Geometry, uniform with his translation of Hirsch's Algebra.

Mr. Babbage has nearly completed for publication, a table of the logarithms of natural numbers to seven figures This work was undertaken for the use of the Trigonometrical Survey of Ireland, and has been, we understand, corrected with the greatest care, and several errors have been detected, which run through almost all known tables.

NEW PATENTS.

To James Whitaker, of Wardale, near Roachdale, for improvements in machinery for pressing cardings from woollen or carding engines, and for drawing, stubbing and spinning wool and cotton. —Dated the 24th of April 1827.—2 months allowed to enrol specification.

To Carlo Glugo, of Lyons, now residing in Fenchurch-street,

 loom,

loom, &c. manufacturer, for improvements in weaving-machinery.—24th of April.—6 months.

To M. W. Lawrence, of Leman-street, Goodman's Fields, for improvement in refining sugar.—28th of April.—6 months.

To J. A. Berrollas, of Great Waterloo-street, Lambeth, for a detached alarum watch.—28th of April.—2 months.

To R. Daws, of Margaret-street, Cavendish-square, for improvements on chairs or machines calculated to increase ease and comfort.—28th of April.—6 months.

To T. Bradenback, of Birmingham, for improvements in bedsteads.—28th of April.—6 months.

To B. Somers, of Langford, Somerset, M.D. for his improvements in furnaces for smelting.—28th of April.—6 months.

To W. Lockyer, of Bath, for his improvement in the manufacture of brushes, and materials applicable thereto.—28th of April.—6 months.

To H. Knight, of Birmingham, for a machine for ascertaining the attendance to duty of any watchman, workman, or other person; also applicable to other purposes.—28th of April.—6 months.

To John M'Curdy, Esq. of Cecil-street, Strand, for improvements, communicated from abroad, in the rectification of spirits.—28th of April.—6 months.

To J. Browne, and W. D. Champion, of Bridgewater, Somerset, for a composition or substance which may be moulded into bricks, or blocks for building, and also made applicable for ornamental architecture.—5th of May.—2 months.

To D. Bentley, of Eccles, Lancashire, for an improved carriage-wheel.—8th of May.—6 months.

To T. P. Coggin, of Wadworth, near Doncaster, for a new or improved machine for the dibbling of grain.—19th of May.—2 mon.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·28 April 8. Wind NE.—Min. 29·44 April 21. Wind NE.
Range of the mercury 0·84.
Mean barometrical pressure for the month . . . . 29·960
Spaces described by the rising and falling of the mercury . . . 4·040
Greatest variation in 24 hours 0·330.—Number of changes 14.

Therm. Max. 70° April 30. Wind S.—Min. 35° April 25. Wind N.
Range 35°.—Mean temp. of exter. air 51°·07. For 30 days with ☉ in ♈ 50·58
Max. var. in 24 hours 22°·00—Mean temp. of spring water at 8 A.M. 49°·78

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 9th . . . . 88°
Greatest dryness of the air in the afternoons of the 14th and 15th . 42
Range of the index 46
Mean at 2 P.M. 55°·5—Mean at 8 A.M. 63°·6—Mean at 8 P.M. 68·8
—— of three observations each day at 8, 2, and 8 o'clock . . 62·6
Evaporation for the month 2·35 inch.
Rain near ground 1·910 inch.—Rain 23 feet high 1·785 inch.
Prevailing Winds N.E. and S.E.

Summary

Summary of the Weather.

A clear sky, 4; fine, with various modifications of clouds, 13; an overcast sky without rain, 8½; foggy ½; rain, 4.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
13	9	28	4	17	18	14

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
4	5	3	5	4	1	4	4	30

General Observations.—The first part of this month to the 16th was mild and generally dry; but the latter part, excepting the last three days, was cold, with frequent showers of rain and hail, and hoar-frost in the mornings. The mean temperature of the air was so low on the 22nd, 23rd, 24th, and 25th, that it became necessary to resume office fires, which had been dispensed with from the beginning of the month.

On the 24th we had several smart showers of pulpy hail without icy nuclei, sufficient to whiten the surrounding hills; and in the northern parts of Hampshire the hills were lightly covered with snow: on the same day the snow on Malvern hills in Worcestershire, and on Blagdon hills in Somersetshire, is said to have been several inches deep. It was brought on by the union of a Westerly wind with an upper current which had blown several days from the North-east. During one week of the low temperature and frosty mornings the spring lay dormant, and no progress appeared in the bloom of the fruit-trees.

The Swallows returned here on the 13th, but from the change in the weather they were very little on the wing for a fortnight afterwards.

The Barometer has been remarkably steady this month, on some days it was quiescent; and the number of changes is comparatively few for April. The mean temperature of the external air for this period is nearly one degree higher than the mean of April for the last eleven years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two *parhelia*, three solar and three lunar halos, four meteors, and two gales of wind from the North-east.

REMARKS.

London.—April 1. Gloomy. 2. Cloudy. 3—9. Fine. 10. Cloudy, with slight showers. 11. Cloudy, and fine. 12. Rainy. 13—15. Fine. 16. Cloudy, and fine. 17. Showers of hail and rain. 18. Rainy. 19—21. Cloudy. 22, 23. Cloudy: cold wind. 24. Hail-showers. 25. Fine. 26. White frost: fine. 27. Fine: white frost. 28—30. Fine.

Boston.—April 1, 2. Cloudy. 3—5. Fine. 6. Fine: thermometer 72° 1 P.M., rain at night. 7—9. Fine. 10. Rain. 11. Cloudy. 12. Cloudy: rain P.M: 13, 14. Fine. 15. Cloudy: rain P.M. 16, 17. Cloudy. 18. Rain. 19. Cloudy: rain A.M. 20—23. Cloudy. 24. Rainy: rain P.M. 25. Cloudy. 26—29. Fine. 30. Fine: thermometer 78° 2 P.M.

Penzance.—April 1. Rain: clear. 2. Showers: fair. 3—5. Fair. 6. Misty rain. 7. Clear: rain. 8. Clear. 9. Fair: rain. 10. Misty rain: fair. 11. Clear: fair. 12. Fair: clear. 13—19. Clear. 20. Fair: rain at night. 21, 22. Rain: fair. 23. Fair. 24. Fair: showers, sleet and hail. 25. Showers: clear. 26. Clear. 27. Fair: clear. 28, 29. Clear. 30. In general clear.

Summary for the Year 1826, *of the state of the Barometer, Thermometer, &c. in Kendal. By* S. Marshall, *Esq.*

1826.	Barometer. Max.	Min.	Mean.	Thermometer. Max.	Min.	Mean	Quantity of Rain in Inches.	No. of rainy Days.	Prevalent Winds
1st Month	30·16	29·23	29·81	44°·5	9°	30°·5	1·821	5	NW.
2d Month	30·00	28·89	29·53	51		41·37	10·775	20	SW.
3d Month	30·27	29·25	29·74	66		41·13	2·255	10	NE.
4th Month	30·78	28·85	29·76	60		45·85	2·749	13	SW.
5th Month	30·10	29·63	29·8	76		51·87	0·369	5	N.
6th Month	30·25	29·70	30·02	85		61·74	0·753	4	SW.
7th Month	30·15	29·34	29·76	81		61·62	3·550	14	W.
8th Month	30·03	29·37	29·73	75·5		59·97	4·600	16	S.
9th Month	30·03	29·16	29·68	68		53·68	3·452	13	W.
10th Month	29·99	29·02	29·63	65		48·69	4·362	19	SW.
11th Month	30·31	28·69	29·61	50		37·15	4·296	11	NW.
12th Month	30·29	28·62	29·61			40·24	4·078	17	SW.
Average.			29·73			47·81	43·060	147	

The preceding Summary of the Meteorological Phænomena for 1826, presents, in most respects, unusual results for this part of the country. The barometer throughout the year has maintained an altitude not very common for the height of Kendal above the level of the sea. This will appear from the mean altitude for the three preceding years: that of 1823 being 29·56; for 1824, 29·26; and for 1825, 29·64. The mean temperature 47°·81 is also greater than in these years. This is a circumstance which has been generally experienced in every part of the island. In 1823, the mean temperature was 45°·00; in 1824, 46°·83; and in 1825, 47°·49. It is generally admitted that no parallel to the late summer can be found for the last 63 years, for intense heat and dryness. In this instance, as in the year 1763, the drought of the summer has been succeeded by an unusually mild and open Autumn and Winter, so far as the latter season has advanced. To the last day of the year, vegetation has maintained much of its verdant appearance, and cattle in this part have been enabled to derive the greater part of their sustenance from the fields. The dryness of the year is sufficiently proved from the circumstance, that only 43·060 inches of rain have fallen in that period, and within 16 miles of this town (Yealand) but 29½ inches have fallen. In 1825, 59·973 inches of rain were taken at Kendal, and in the three preceding years, of 1822, 1823, and 1824, nearly 63 inches fell in each year.

In this town the winds from the S.W. may be said to prevail during nine months in the year; but in 1826, but five months show this wind to have been the most prevalent. As it is from this quarter that the greatest quantity of rain accompanies the currents of the atmosphere, this circumstance appears to be an additional proof, (if any were wanted,) that the greatest quantity of moisture is conveyed by this wind. In 1823 we had 198 rainy days; in 1824, 187; and in 1825, 169; but in 1826, we have had only 147 days in which rain has fallen; and had it not been for the remarkably wet month of February, the number would have been much smaller. S. M.

Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1827.		Barometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Gosport. Max.	Gosport. Min.	Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Gosport. Max.	Gosport. Min.	Bost. 8 A.M.	Wind. Lond.	Penz.	Gosp.	Bost.	Evapor. Lond.	Gosp.	Rain. Lond.	Penz.	Gosp.	Bost.
April 1		30·34	30·29	29·97	29·96	30·15	30·10	29·84	49	44	54	44	53	46	45·5	SW.	W.	NW.	W.	...	...	0·02	0·750	0·180	...
2		30·29	30·24	29·94	29·88	30·10	30·10	29·73	48	38	55	48	61	45	51·5	W.	NW.	W.	W.	...	...	...	...	...	...
3		30·27	30·24	29·88	29·86	30·09	30·08	29·60	58	44	53	48	61	48	54	NW.	W.	W.	W.	...	0·40	...	...	...	...
4	☽	30·25	30·24	29·84	29·80	30·11	30·07	29·68	63	44	54	47	65	49	55	SW.	W.	W.	W.	...	...	...	...	...	...
5		30·24	30·10	29·74	29·70	30·05	30·00	29·65	67	43	55	47	59	47	53	SW.	SW.	SE.	W.	...	...	...	...	...	...
6		30·26	30·09	29·70	29·64	30·02	29·92	29·45	72	49	56	47	62	46	58	SW.	W.	W.	S.	...	·15	·14	...	...	...
7		30·48	30·26	29·90	29·80	30·23	30·12	29·75	61	45	54	44	59	44	52	NW.	W.	NW.	W.	...	...	...	...	...	0·07
8		30·48	30·25	30·00	30·00	30·28	30·23	29·99	63	43	56	42	63	43	53·5	SE.	NW.	SE.	E.	...	...	...	0·320	...	...
9		30·25	30·05	29·82	29·70	30·10	29·95	29·62	61	49	53	46	56	45	54	SW.	SW.	NW.	SW.	...	·15	...	...	·010	...
10		30·06	30·05	29·62	29·60	29·88	29·87	29·42	60	46	50	47	63	49	50	NW.	N.	N.	calm	...	...	·06	...	·160	...
11	○	30·05	30·00	29·62	29·60	29·88	29·88	29·50	62	46	55	41	61	46	45·5	SE.	NW.	SE	calm	0·98	...	...	...	...	·14
12		30·25	30·00	29·64	29·50	29·90	29·79	29·37	55	37	55	49	57	39	48·5	SW.	W.	S.	calm	...	·10	·22	...	·360	...
13		30·35	30·25	29·90	29·88	30·15	30·10	29·65	65	35	54	43	61	42	51	NW.	NW.	SW.	SW.	...	...	...	...	...	·15
14		30·25	30·20	29·96	29·94	30·20	30·15	29·80	62	43	55	43	60	41	48·5	NW.	E.	N.	NW.	...	...	...	...	...	...
15		30·21	30·20	29·94	29·90	30·08	30·06	29·62	63	44	57	41	61	42	49	NW.	E.	N.	calm	...	·25	...	...	...	...
16		30·20	30·19	29·88	29·84	30·05	30·04	29·70	58	37	56	44	61	41	44·5	NE.	NE.	NE.	N.	...	...	...	...	...	·18
17		30·19	30·10	29·78	29·74	30·04	29·98	29·74	59	40	54	43	58	42	44·5	NE.	SE.	NE.	calm	...	...	·28	...	...	...
18	☾	30·10	29·94	29·70	29·68	29·95	29·84	29·55	51	40	55	40	52	46	43	NNW.	SE.	NW.	NW.	...	·15	·05	...	·035	·07
19		29·94	29·86	29·60	29·50	29·76	29·68	29·44	51	33	56	43	60	40	47	NE.	SE.	E.	NE.	...	...	...	...	...	·39
20		29·86	29·73	29·40	29·30	29·62	29·52	29·35	54	34	55	44	57	44	48	E.	S.	E.	calm	...	...	·01	...	·325	...
21		29·87	29·73	29·38	29·20	29·54	29·44	29·35	51	42	55	43	57	43	45·5	NE.	NW.	NE.	E.	...	·15	...	...	·760	...
22		29·89	29·87	29·58	29·48	29·73	29·66	29·54	45	37	49	44	50	39	44	NNE.	NW.	NE.	NE.	...	...	·10	...	...	...
23		29·89	29·74	29·58	29·44	29·70	29·63	29·54	42	31	49	40	48	40	42	NNW.	SE.	NE.	N.	...	...	...	...	·050	...
24		29·92	29·74	29·50	29·40	29·65	29·52	29·35	50	32	48	39	50	37	38·5	SW.	NE.	W.	SE.	...	·20	·02	...	·030	·07
25		30·29	29·92	29·72	29·60	29·92	29·78	29·35	55	24	51	36	51	35	41	SW.	W.	W.	W.	...	...	...	...	...	·11
26		30·46	30·29	29·84	29·82	30·25	30·10	29·87	62	30	51	36	54	40	46·5	NW.	E.	S.	W.	·90	...	...	0·290	...	...
27		30·46	30·26	29·86	29·80	30·26	30·15	30·05	64	33	55	43	55	46	50·5	SE.	SE.	SE.	S.	...	·40	...	...	...	...
28		30·26	30·18	29·68	29·68	30·06	29·94	29·82	76	38	56	48	62	50	55	E.	SE.	E.	SE.	...	...	...	...	...	...
29		30·19	30·18	29·70	29·68	30·03	29·98	29·66	78	40	61	48	64	48	60·5	SE.	NW.	SE.	calm	...	...	...	...	...	...
30		30·18	30·17	29·84	29·80	30·07	30·05	29·63	79	53	61	48	70	50	67·5	SE.	NW.	S.	W.	·56	·40	...	...	...	...
Aver.:		30·48	29·73	30·00	29·20	30·28	29·44	29·41	79	24	61	36	70	35	49·5		NW.			2·04	2·35	0·90	1·360	1·910	1·18

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END OF THE FIRST VOLUME.

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